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Vol. 8, No. 3 September, 1954

Canadian Journal of Psychology

THE NEW ECLECTICISM[1]

D. C. WILLIAMS

University of Toronto

BRETT's comment that psychology has a long past but a short history underlines at once man's speculative concern with his own nature as a persistent characteristic of recorded philosophy, and his centuries-old reluctance or inability to attempt an objective approach to that concern. Indeed it has often been remarked that, historically, the development of the various scientific disciplines is in inverse ratio to their nearness to man himself as an object of study.

In revolt against the armchair speculation of the past, psychology was (and still is) enjoined, "Get thee to a laboratory, and quickly too," though it is remarkable that the effect of taking this advice seriously was to produce even more theory in the battle of the schools. Without labouring the obvious at too great a length, it seems to me that the upshot of this was of a twofold nature. First, the winners on this continent were neo-behaviourism and neo-Freudianism; one may suspect that the modifications symbolized by the prefix "neo" in each case had many of their roots in their ancient antagonist, Gestalt psychology. Secondly, there was the widespread adoption of eclecticism as a simple-minded, though anything but simplifying, way out of the dilemma.

Less obvious but perhaps most important is the development of the present more sophisticated eclecticism which, I think, is manifest in the current return to the armchair, a device rapidly becoming as important to psychology as is the couch to psychoanalysis or the bed to Kinsey. "Eclectic" a few years ago was a term of contempt, operationally and sulphurously defined as a person who borrowed theoretical catch words indiscriminately and with no regard for their logical consistency; today, however, eclecticism has become the search for an underlying unity of principle hidden by the diversity of language, plus a new awareness of the way in which communication is biased by the theoretical presuppositions which dictate both its polemic and its experimentation. I will not bore you by citing a whole litany of literature to document this assertion, but Marx's text *Psychological Theory* and Brunswik's monograph *The Conceptual Framework of Psychology* come readily to mind, as does the symposium on *Theoretical Models and Personality Theory* (eds. Klein and Krech), to say nothing of the January, 1953, issue of the *Psychological Review*, which was almost swamped by the subject.

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[1] Presidential address, delivered at the Annual Meeting of the Canadian Psychological Association, Montreal, June 6, 1954.

We have, it seems to me, drastically and advantageously shifted our ground. Whereas one previously adopted a theoretical position and argued it from *within* that frame of reference, now we realize the advantages which may accrue if any number of given positions are found to be approachable from *without* the particular frame of reference each espouses. It has occurred to many of our number that merely to criticize Theory A from the vantage point of Theory B is unlikely to produce any other effect than a rejoinder in kind and so ad infinitum. This inner-vs.-outer orientation may be illustrated by two specific examples from recent literature. Dallenbach (7), writing about the role of theory in psychology, categorically states that no theory was ever upset by mere argument, that facts alone decide the issues. This contrasts sharply with Boring's position in a recent article on the place of theory in experimental psychology (4) in which he concludes in effect that theories are "all ye know and all ye need to know."

The Dallenbach position typifies the attitude of classical experimentation. But a glance at learning theory, for example, makes it clear that "facts" are available for the support of any intelligently conceived theory. Similarly the "facts" of the relative-vs.-absolute choice of experiments have been incorporated into the theoretical positions of both Koehler and Spence, to the apparent satisfaction of each. The chicks continue to get food, and in the process seem to be able to nurture conflicting interpretations of how they do it. All of which indicates that the appeal to fact does not necessarily resolve our theoretical dilemmas. The reason, I suggest, is that such an appeal is still encapsulated within the specific theoretical framework being urged upon us.

Boring's article illustrates an alternative approach. Grounding himself in that impressive historical perspective for which he is justly famous, he shows that today's facts are simply yesterday's concepts, reified by the versatility and consistency with which they fit so many theories and observations. The possibility of reaching so clear-headed a conclusion stems from his adoption of a non-theoretical—in this case, an historical—approach to the problem. It is this current concern to find an alternative, unbiased vantage point from which to view dispassionately the whole field of theory which I term *the new eclecticism.*

Whence this emphasis on eclecticism and whither has it led? It comes, I think, from several sources. Whereas each antagonist in a theoretical duel is forced into an all-or-none position with respect to his theory and that of his opponents, the eclectic, with no such ego-involvement, may be genuinely impressed by apparently valid though mutually exclusive arguments on both sides, and is stimulated thereby to seek a wider context on a different level of abstraction into which both can be made to fit. To think in this way is to think that theoretical psychology is susceptible of unity and coherence, and further that such unity of principle is attainable.

This admirable though vague ideal has recently been clarified and made more precise by a growing concern with the tools of the theorist's trade. What is involved in theory making? How do you go about it? What is at stake in having a theory? How is it communicated? What is it supposed to do? How comprehensive must it be? These and a host of similar questions now agitate the minds of all who have the slightest concern for the unification and coherence of our discipline. This is armchair stuff, though no longer devoted to the introspective contemplation of the psychological navel. The armchair process is essentially a supplement to, rather than a substitute for, experimentation.

What then has the armchair turned up? Quite a bit. It has, oddly enough, turned up operationism, a methodological tour de force which broke the sound barrier of meaningless linguistics in its insistence on the final behavioural "pointing to" in the clarification of concepts. Similarly, logical positivism has made us keenly aware of the internal structure and formal properties of the grammar of science, and Bergmann, for example, is quite prepared to act as philosophical father confessor to a host of busy experimentalists who don't want to be caught with their logical slips showing.

The widespread acceptance of Tolman's distinction between molar and molecular levels of analysis, and the Meehl and McCorquodale emendation of his statement of the intervening variable (from which must be distinguished the hypothetical construct), all point to an increasing rapprochement, a widening and clarifying of the conceptual frame of reference within which communication at an appropriate theoretical level becomes possible. Now we are all methodological behaviourists, says Bergmann, and who arises to doubt him? We are all positivists, says Tolman in his autobiography, and those who have reservations at least keep their counsel, as all underground movements must.

It is of course true that these methodological considerations produce general agreement on the rules of the game rather than general acceptance of a specific theoretical position. They produce, as it were, a *modus vivendi* without cordiality. Hence there remains plenty of room for controversy within this ambit, but again I suspect that it is the type of controversy which moves us to the cynical "this is where I came in" comment. It is controversy essentially designed as a recruiting campaign to rally the faithful beneath a given banner, and this, however admirable from the proselytizer's viewpoint, remains a necessary but not a sufficient condition for the development of a coherent psychology.

Accordingly I will invite you to consider some of the other discoveries about what is involved in "having" a theory, since this area is by no means exhausted by the methodological triumphs just referred to. As I see it, the critical shift in emphasis is reflected in the current concern with treating the *process* of theory-making as an object of study, whereas our earlier object was the completed theory or the reported "fact." The literature of the past few years shows plainly that we are examining theory-process again for fresh insights.

While the term theory has many uses and these are susceptible of categorization in many ways, I propose to distinguish three meanings which seem to be in common usage:

1. Theory as explanation, e.g. Freudian theory, theory of ionization, etc.—designed to explain or account for a set of interrelated phenomena in a particular way and at a particular level.
2. Theory as an area—personality theory, economic theory, etc.
3. Theory as distinct from non-theory, e.g. from fact or method—here we are concerned with the characteristics of theory as such.

Theory in the first sense is specific in content and designed as explanatory; in the second it is spatial, designating an area; in the third it is temporal, concerned with process.

Most of us began with specific content theories as explanations, but the immense complexity of psychology soon drove us in despair to the second position, where we set up courses of personality theory, learning theory, emotion, motivation, and the like, and invited our students to share our continuing confusion under the guise of an impartial presentation of all points of view—all very democratic and eclectic, in the worst sense of those terms. Now we are beginning to realize that the third view offers a different level of analysis, one that has proved fruitful and can therefore be drawn on again with some confidence.

At long last, then, I turn to a consideration of models. To conceive of the process of theory-making is to enter the realm of creativity itself. Small wonder that Guilford (**9**) should have devoted his brilliant presidential address to this topic. Small wonder, too, that Bakan (**3**) should enter a plea for distinguishing in learning experiments between "what is taught" and "what is thought." The former is the basis of all learning investigation, since the psychologist creates the problem and defines learning as the ability of the subject to perceive and act on what he put there. How can these data illuminate "what is thought," i.e., the creative learning of the psychologist who devised the experiment *ab initio* and without such guidance?

How then do we invent, create, or otherwise come by a theory? Whatever the final answer, the process inevitably involves reliance on models of one kind or another. Here again the model as such has become an object of study, and the effort is proving to be revealing. What is immediately revealed is that familiar chaos which is generated when one is forced to discriminate as *figure* something which has always existed in the mind, but only as the uncritically accepted *ground* for the conduct of thought.

We suffer in the field of models from a profusion of choices. Phenotypically they are as boundless as nature itself and as pervasive. We draw on clocks, switchboards, hydraulics, plumbing, icebergs, rats, babies monkeys; on economics, physiology, history, culture, and society. This wealth of models illustrates at once their utility in the conduct of thought and their power to confuse by the variety of their origins and functions.

To add to the fun, many writers write as if theory were logically prior to model; others reverse this priority, and others use the terms synonymously. Then too some select a general model and stick to it, while others change models as regularly as they change their minds. This thoroughness of approach is characteristic of psychology; once it becomes confused it never rests until all possible stones are upturned, so that all possible seekers after knowledge will trip over something.

Fortunately the notion of process itself prevents us from falling into the more obvious traps indicated above. It is not necessary to enquire which is "right" and which "wrong," nor even, I suggest, to determine which really has the logical priority, when it is demonstrably apparent that psychological priorities can be assigned at will, with no more than irritant effects on those who think in reverse-order terms. Instead, it seems to me to be more profitable to ask: Whence these models? What are they? What do they do? And even, perhaps, why models?

One of the most illuminating discussions of the origins of models is to be found in an article by Karl W. Deutsch (8), who has recently become interested in communications and cybernetics. It is, I think, reasonably clear that an interest in communications when applied to theorizing in any field must inevitably direct one's attention to the concept of models, since they are adopted primarily for purposes of communication. Further, their characteristic properties, once elucidated, show clearly that they often function to limit the extent to which theoretical notions can be communicated, or at best introduce a bias into the way in which phenomena are to be observed and valued. Again, a useful model, generated in a field of high scientific prestige, tends to be tried on for size in a host of fields widely separated in data, methodology, and rigour from that of its point of origin. The homeostatic model is a case in point; originating in physiology, it rapidly spread through the psychology of motivation and on to that old standby, perceptual constancy. It has enjoyed a similar run in economics, which is perennially concerned with "balance" models in its examination of trade and commerce, and on more vaguely into sociology and political science, where it turns out to be at best politically ultraconservative, if not downright totalitarian in conception.

The problem of the origins of models is susceptible of a most interesting historical analysis. As Deutsch has shown, man continually relies on the technological aspects of his social organization for models for the conduct of his thought. The current concern with the hierarchical organization of large institutions, be they commercial, military, or governmental, is dominated by the ancient model of the pyramids of Egypt. Like them the modern institution rises to its apex in the lordly figure of the president, chief of staff, or Prime Minister, supported in office by successively descending ranks of lower echelons. The model is static and inflexible, as is much of the thinking and doing that goes into its operation and, one is tempted to add, into much of the psychological analysis of that operation.

Similarly, no one can think of politics, be they international or intraprofessional, without having recourse to the wheel as a conceptual model for those changes analogous to the wheel's essential function, i.e. revolution. Again, fortune's wheel or fate is a centuries-old "explanation" of man's affairs, and, projected to the skies, the model encompasses the spheres and epicycles of Ptolemy's cosmology. The wheel clearly reflects the perceived need of a dynamic model, but fails because its dynamics are not inherent in its structure, but must be supplied from without the system.

These are but two of the precursors of that great triumph of 18th century model-making, the clock, which symbolizes, in the exact meaning of that term, the whole flowering of mechanical models and hence of mechanism itself. Equipped with a pump as model, Harvey was finally able to make sense of the circulatory system. After Newton, and with the addition to mechanism of the principle of parts operating at a distance, the universe was conceived as a vast clockwork, a concept which provided the navigator with a direct connection through his chronometer between the stars in their courses and the ship in its course.

It is surely not stretching the case too far to see in the clockwork model the basis of the scientist's methodological passion for precision in prediction; the prediction *that* an event will occur is not nearly as impressive as the additional specification of *when* it will occur, or the still more impressive experimental variant of controlling conditions so that it can be made to occur *now*. Hence, too, the importance of accurate measurement, of precise quantification, and so on. To bring the clockwork's influence more directly into the psychologist's domain, one need only recall the mechanistic implications of Darwinian theory and note, as Asch has shown, how early behaviourism slavishly paralleled this model. Asch writes: "Learning is conceived as a process of selection of an adaptive pattern which takes place in the nervous system. Habits are said to emerge analogously to body organs, from a competition in which the survival of the fittest does not depend on anyone's perception of that fitness" (**1**, p. 15).

These few examples may serve to clarify certain of the formal properties or characteristics of models generally, and of the way they are used. It is clear that a model is always a representation of reality, not the reality itself. The latter is conceived to be too complex, too vast, too difficult to deal with, and hence must be attacked in the simpler form of the model. It follows that the model must be such that it can be regarded as being related in some way to the reality it represents. It may be an idealized form of that reality, as in the ideal-type model of the historian, or the physicist with his "pure case," never realized in practice; or it may be a working model, a "slice of life," wherein we arbitrarily abstract certain elements from a situation, call them critical (or essential, or natural) and study their interaction. Bavelas' justly famous investigations of the effect of the form of the network on the process of communication is an excellent case in point. These two in effect are what we usually mean when we discuss formal vs. material models. Nor are they entirely irrelevant to the recent discussions on the differences between intervening variable and hypothetical construct, since the latter carries with it the promise of identifiable surplus meaning.

Historically, models are essentially analogical in function and hence the broadly accepted cultural models are always structures. Deutsch makes the point that all structural models have a formal model "behind" them, as it were, but it does not follow that all formal models necessarily result in structures. Here a good deal of debate ensues. Von Bertalanffy (**14**) feels we are better off for the moment to stick to formal models, pointing out what Mendel did with one in the absence of a structure; whereas Krech and Klein (**11**) argue that meanwhile our knowledge has increased, and that what was good enough for Mendel in 1784 is not good enough for von Bertalanffy in 1953. On the contrary, Bush and Mosteller (6) argue for the formal mathematical model as semantically pure, highly useful, and eminently respectable. The cyberneticists, of course, insist upon structural models; having already solved the formal mathematical equations of information theory, they set up a new definition of reality for themselves. The old persuasive certainty implied in seeing something "in the flesh" has given way to the ambition of realizing a formula "in the metal." Hence the building of machines which "think," "remember," and so on.

Ross Ashby (2) carries this ambitious model-making a step farther when he not only sets up a descriptive model for a brain, but formalizes his critical steps in mathematical terms, and then invents a "homeostat," a gadget whose functions he says *are* brain functions. His concern is to produce the "perfect" model situation; that is, to be so rigorous in his derivations and homeostat demonstrations that he can tell us precisely how the human brain *must* function. This, I think, goes out of bounds and violates the essential rules of models and their functions, a point to which we shall return later.

The great appeal of mechanical models is their apparent dynamic quality. The parts interrelate, and homeostasis or balance or equilibrium is attained when moving parts or opposite forces tend to equate each other. For example, the aeroplane achieves dynamic equilibrium when thrust equals drag and lift equals weight. But this condition obtains only when the aeroplane is in straight and level flight. The consequence of the search for such dynamics in psychology has been that we have become more and more concerned with the internal workings of our models of human behaviour, taking account of environmental effects only in terms of specific stimuli impinging on and altering the balance of the internal reciprocal mechanism.

As Brunswik (5) says, "the confinement of nomothetic endeavor to the internal aspects of the system as seen under the impact of external influences is strikingly similar to the encapsulated theorizing and model-making in most of modern theoretical psychology." Such considerations raise, too, the problem of whether it is possible to achieve a dynamic model without postulating its motivation in terms of mutually contradictory principles, as Freud did. Given such inherent contradiction, the concept of conflict is inevitable, and the possibility of unity at the level of personality becomes remote indeed.

The machine model, then, is an encapsulated model inviting its devotees always to a progressively more detailed examination of its internal, more delicate and elusive part-functions and structures, and furnishing in the process the logic of reductionism. "Reality" always lies at that layer of analysis which just eludes the experimenter's present equipment for investigation. I am not attempting to argue this model out of existence; it is too helpful for that. My purpose is the more modest one of drawing attention to the fact that the model, as historically given, and often as scientifically espoused, is repeatedly and uncritically identified with reality, particularly if model and theory are regarded as identical terms. This confusion of reality and model becomes well nigh inevitable if one is not aware of the concept "model" at all, and uses the term "theory" indifferently to describe both. To recognize a model when we use one is to recognize that it has certain limitations. To fail in such recognition is to conceive of one's theorizing as deriving directly from the principles of nature, and hence to identify such theory with a law of nature. As such it becomes a "discovery," whereas it is really an "invention," a man-made way of conceptualizing some aspect of the universe. Every schoolboy believes that Newton discovered the law of gravitation, but as it turns out, he invented a model of the universe instead. The concept "model" has as its main purpose the underlining to man of the limitations of his intellect in its ceaseless quest of reality. You recall Archie, the *vers libre* poet whose soul was pent in the body of a cockroach, and you may also recall his frustration at so unhappy a dispensation, when he says, "If I were to plan out a drama, great as great Shakespeare's Othello, it would be touched with the cockroach and people would say it was funny." The poet is inexorably limited by the cockroach. Similarly man's theorizing is "touched with the human," and who knows what laughter it evokes on Olympus?

A clear awareness of the role of model is helpful in explaining the curious paradox whereby scientific theory, that most fragile and intangible product of man's thought, is so often grasped and defended with such extraordinary emotional strength and tenacity of belief. Nor are stubborn facts to the contrary likely to have the slightest effect in forcing a reorganization of this structure; they are merely explained away. This familiar state of affairs is only possible when theory is regarded as discovery. Thus oriented, the theorizer conceives of his theory as the actual embodiment of a law of nature. Theory then becomes identical with reality, and, being real in this ultimate sense, it follows that it must also be truth. It cannot then be challenged with impunity. To impugn the theory is to impugn not only its defenders' deepest faith, but also the Creator himself who, by definition, must have invented the theory to begin with.

This delicate, and to the unwary questioner, perilous situation is avoidable if we begin on the premise that theories are created, made up, invented, by human beings to account for certain observed regularities and sequences in nature. Since we seem unable to grasp natural phenomena in all their complexity, we have recourse to models, made up of selected aspects of such reality, as vehicles for the conduct of our theorizing and investigations. To test the theory is to put the model to work. Correspondence now is correspondence of findings and model of reality, not of findings and reality *per se*. If small divergences are found on empirical appeal the model is conceived to be able to tolerate them. If serious discrepancies occur the model may be revised or discarded and this may involve a radical alteration of theory. But in principle, at least, all this may be done without injured feelings, insult, and all the alarm reactions of the person whose reality system is threatened. With the model as concept, it becomes possible to conduct theoretical investigation at a constant semantic and conceptual level, instead of engaging in angry debates all up and down the structural differential. Either questioner or originator can attack a model with impunity because it is recognized as an unreal, arbitrary, and man-made affair. To ignore it in theorizing is to exemplify the insight of Artemus Ward when he said, "Ignorance ain't so much a matter of not knowing nothin' as it is matter of knowin' so many things which ain't so!" Ashby's attempt to identify his model with brain function is thus a clear case of the misuse of the model concept, as when he says the brain *must* work in such and such a manner.

To date I have defended the armchair through a cursory examination of theory, noting its utility in directing our attention to the process of theory-making, and showing how an extra-theoretical position emerges as a necessity for such operations. In examining that process I have attempted to show where the concept "model" fits in and to indicate some of its characteristics. It remains only to deal with the role of phenomenology in this process, and this need not detain us long. I have suggested that the new eclecticism seeks always an alternative vantage point, of necessity outside the theory area it would contemplate, in order to appraise the process of theory-making itself. I have rejected within-theory criticism as essentially incapable of functioning in this fashion. I have said that logical positivism is such an outside vantage point position, as is operationism. The historical perspective is another, and an enlightened common sense, such as that displayed by Harlow (**10**) in his "Mice, Monkeys, Men and Motives" is a fourth. It is quite obvious that phenomenology is going to turn up as a fifth offering, and that I suspect it to be the best of the lot.

The argument in its support is clear, and MacLeod (**12**) has put it admirably. If we are all methodological behaviourists and positivists in our experimentation, we are also all phenomenologists as sentient human beings. We live in a world of phenomena, a world where the fact of consciousness is logically prior to all other facts. We should therefore accept this inescapable priority and use it advantageously in the contemplation of the current state of theoretical psychology. This is difficult because it calls for ridding ourselves of bias, a well-nigh impossible task, but one which can be approached by admitting and noting the biases we uncover in the effort. MacLeod in any case has obligingly listed an impressive array of them as an aid to beginners. This discipline is precisely what the new eclecticism needs if it is to avoid the pitfalls of the old.

Again, the phenomenological position need not upset the most committed of believers in any of the current theoretical positions, since it is not and cannot become a coherent, exclusive psychological theory itself. MacLeod is emphatic on this point. You can be anything you like *and* a phenomenologist without inconsistency, since phenomenology does not operate at the same level of discourse as psychological theory. Further, by dealing with the phenomena of psychology one can escape the model bias itself, thus avoiding the explicitly stated dilemma of Brunswik who has to use a model (the lens) in order to examine other models.

The phenomenological position has the advantage over the historical in that it encompasses historical data without being subject to history's *lacunae;* it has the advantage over positivism and operationism in that it begins with the experiencing subject, which they never reach; and it surpasses that uncommon commodity, common sense, by providing the latter with a coherent frame of reference in place of its usual arbitrary rules of thumb.

Phenomenology starts with the fact of consciousness, but psychology, keenly aware that the vocabulary of physics taboos such subjectivism, has bowed to the taboo in much of its systematic thinking. I am indebted to an engineer for a bold and, to me, novel approach to this dilemma. Arnold Tustin, Britain's foremost expert on information theory, simply throws the gauntlet down to physics, regarding the whole psychological problem as a failure on the part of that august discipline, physics, to develop a conceptual vocabulary adequate to the psychologist's needs (**13**). It is hard to see how the acceptance of this challenge could be undertaken on other than phenomenological grounds.

One final example. Phenomena vary with subjects, a fact well attested to by the situation of the speaker and his audience. To the speaker there is never enough time to embody and explore all the aspects of his thought; for him time flies on phenomenal wings. Yet how often is that same speech nothing but a grim foretaste of eternity to his long-suffering audience!

REFERENCES

1. Asch, S. E. *Social psychology.* New York: Prentice Hall, 1952.
2. Ashby, W. R. *Design for a brain.* New York: Wiley 1952.
3. Bakan, D. Learning and the scientific enterprise. *Psychol. Rev.*, 1953, **60**, 45–49.
4. Boring, E. G. The place of theory in experimental psychology. *Amer. J. Psychol.*, 1953, **66**, 169–184.
5. Brunswik, E. *The conceptual framework of psychology.* Chicago: Univer. of Chicago Press, 1952.
6. Bush, R. R. & Mosteller, F. A mathematical model for simple learning. *Psychol. Rev.*, 1951, **58**, 313–323.
7. Dallenbach, K. S. The place of theory in science. *Psychol. Rev.*, 1953, **60**, 33–39.
8. Deutsch, K. W. Mechanism, organism and society: some models in natural and social science. *Philos. Sci.*, 1951, **18**, 230–241.
9. Guilford, J. P. Creativity. *Amer. Psychol.*, 1950, **5**, 444–454.
10. Harlow, H. F. Mice, monkeys, men and motives. *Psychol. Rev.*, 1953, **60**, 23–32.
11. Krech, D. & Klein, G. Theoretical models and personality theory. *J. Personal.*, 1951, **20**, 2–23.
12. MacLeod, R. B. The phenomenological approach to social psychology. *Psychol. Rev.*, 1947, **54**, 193–210.
13. Tustin, A. Lecture delivered at University of Toronto, March, 1954.
14. von Bertalanffy, L. Theoretical models in biology and psychology. *J. Personal.*, 1951, **20**, 24–38.

ECCLES' NEUROPHYSIOLOGICAL MODEL OF THE CONDITIONED REFLEX[1]

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J. C. ECCLES, distinguished pupil of Sherrington, has recently made neurophysiological discoveries of great theoretical importance for psychology. These are summarized in his book, *The Neurophysiological Basis of Mind* (2). I do not propose to deal at length with Eccles' theory of consciousness; as a monist I must perforce reject his dualism. In my opinion Eccles' chief contributions are experimental ones, stemming from the development of an important new electrophysiological method: the micro-electrode which can penetrate and record from within a single nerve cell.

Neurophysiological studies of long-lasting modifications in synaptic function have been needed in order to explain the simplest instances of learning. Eccles and his co-workers are the first to show conclusively that repeated stimulation produces measurable alteration at the synapse, and that this alteration may persist for relatively long periods of time.

Eccles believes that the increased effectiveness of transmission is brought about by swelling of presynaptic fibre and knob, or by the knobs coming in closer apposition to the postsynaptic membrane. Eccles is convinced that present evidence is sufficiently strong to permit the acceptance of some such synaptic change as mediating learning, and the rejection of less plausible mediators, such as long-lasting circulation of impulses in closed chains of neurones.[2] He goes on to present his model of the conditioned reflex, the only model, as far as he is aware, which strictly conforms to the facts of neurophysiology.

Eccles' model shows neural networks rather than the single neurone

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[2] He does, however, bring in prolonged reverberatory activity, occurring in the neuronal network, to explain how a single event (CS and UCS combined) may activate each link in a spatio-temporal pattern thousands of times within a few seconds. He notes that Hebb (3) makes a related postulate when he supposes that "a reverberatory trace might co-operate with the structural change, and carry the memory until the growth change is made."

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type of diagram often drawn in the past. Connections are indicated by columns or bands rather than by single neurones. In addition to the usual conditioned stimulus (CS), unconditioned stimulus (UCS), and response (R), there are two centres: first, the receiving centre (RC) which receives impulses from UCS; this is the centre for the unconditioned reflex. The second, or convergence centre (CC) receives afferent impulses from both pathways (CS and collaterals from UCS). The discharge of impulses from CC will develop a special spatio-temporal pattern of impulses in the neuronal network, which he diagrams as a path forming part of the NN (neural network) column. An ingenious diagram shows how the same neuronal network may respond differently with a change in input (2, pp. 220–222). He goes on to show how such a system can account for acquisition of a CR which is, incidentally, not quite the same as UCR (in conformance with behavioural fact).

Eccles' analysis of the acquisition aspect of conditioning seems correct. But his account of experimental extinction does not appear to be in line with the behavioural facts. He says: "Thus, with CS alone there will be progressively less activation of synaptic knobs in the neuronal network, which will consequently regress; and so an explanation is provided for the gradual extinction of a conditioned reflex when it is not continuously being reinforced by the unconditioned stimulus" (2, pp. 224–225). This of course is the situation where, to use the familiar Pavlovian example, the tone is sounded again and again without subsequent introduction of meat powder into the dog's mouth. It is true, as Eccles says, that in this situation there is progressive weakening of the conditioned reflex, and thus far there is nothing wrong with his analysis. But what happens if, instead of presenting the tone without meat powder, we just leave the animal in its cage and do not present the tone at all for a long period of time? Applying Eccles' principle of disuse (2, pp. 224 f.) should we not expect this situation to produce even more rapid weakening of the conditioned reflex than the first situation (tone without food)? What actually occurs, however, is the exact opposite. In the absence of experimental extinction (CS presented without UCS), CR's persist for very long periods—months, years (4, p. 129). Repetition of CS is necessary to produce a significant weakening of reaction, and, following extinction, failure to present the CS will actually lead to a strengthening of the reaction (spontaneous recovery).

INHIBITION IS THE ELECTRICAL OPPOSITE OF EXCITATION

It is pleasant to record that the most likely correction for the defect in Eccles' CR model is to be found in the experimental work in inhibition carried out by Eccles himself, and his co-workers. Indeed, these discoveries concerning inhibition appear at least as important as the discovery of long-lasting changes at the synapse. Brock, Coombs, and Eccles (1) found that excitation was accompanied by postsynaptic potentials of depolarization, while inhibition was accompanied by the opposite of depolarization, namely, hyperpolarization. We shall now turn briefly to the important question of inhibition in general, coming back subsequently to Eccles' CR model in an attempt to deal with the apparent defect in the original model.

Inhibition is a concept of major importance for any behaviour theory, and Eccles' new findings with respect to inhibition phenomena at the synapse cannot fail to have strong influence on psychological theory. These findings signify, of course, that synaptic inhibition is a phenomenon in its own right, and not merely some kind of a by-product of excitation. At the synapse, inhibition is an active hyperpolarization process in the surface membrane of the motoneurone. Moreover, this hyperpolarization may be regularly and predictably produced in the motoneurone by stimulating its inhibitory branch of the nerve in the reflex preparation for study of reciprocal innervation (2, p. 172).

Sometimes behavioural observations suggest, even demand, the presence of certain physiological mechanisms before they are discovered by the physiologist. Something like this seems to have occurred here. Psychological analysis of conditioning showed that inhibitory phenomena could not be accounted for solely in terms of excitatory principles. This analysis was rooted historically in some keen observations of Pavlov which have been many times confirmed in various conditioning experiments of the American school. Pavlov found that the "inhibitory" effects of the procedure of experimental extinction—repeating the conditioned stimulus without paired presentation of the unconditioned stimulus—diminished with time (when the animal was kept in its cage without presentation of either CS or UCS). He called this "spontaneous recovery of the conditioned reflex." In analysing phenomena of experimental extinction, Hilgard and Marquis (4) concluded that at least two principles were required: interference, "a process in which old learning is antagonized by new," and "adaptation," a process of inhibition whose strength is a direct function of frequency and rate of repetition. It was further postulated that adaptation is not permanent. The phenomenon of spontaneous recovery was one thing which made it necessary to posit the process of adaptation, because spontaneous recovery could not be accounted for in terms of the principle of interference (4, p. 118).

There is a second line of evidence against the interpretation of extinction as interference from new learning. Even with continued reinforcement (CS followed by UCS) the conditioned response (CR) will lose strength under certain conditions, the main one being "massing of trials" (many trials with only brief intervals between paired presentations). Such a phenomenon cannot be accounted for except in terms of an active inhibitory process produced by repeated reaction, the process which Hilgard and Marquis called "adaptation." Hull (**5**, p. 278) has incorporated this process in his behavioural system, giving it the symbol I_R (reactive inhibition), and this term has come into more common use than "adaptation."

Just as Sherrington inferred from his observations of the reflex preparation that there must be a central inhibitory state, independent of the central excitatory state, so Hilgard and Marquis inferred from observations of conditioning in intact organisms (human as well as animal) that there must be an independent inhibitory process (adaptation). The historical importance of Sherrington's work in relation to this problem in the field of conditioning is shown by the authors' references to Sherrington's early work on reflex adaptation (**4**, p. 105) and to his basic work on reciprocal innervation (**4**, p. 109).

ECCLES' WORK ON INHIBITION APPLIED TO HIS CR MODEL

Now let us return to the CR model. Experimental extinction is a procedure leading to reactive inhibition, and in all probability the amount of inhibition from disuse is very slight or nil. The most relevant neurophysiological findings for experimental extinction come from Eccles' studies of repetitive stimulation. Not just excitatory reflex effects, but inhibitory effects too, are strengthened by repeated presynaptic stimulation. We must be cautious in applying these findings directly to phenomena of experimental extinction, because it has not yet been shown that repetitive stimulation can change the direction of effect from excitation to inhibition in the monosynaptic preparation. But inherent in the present findings is a possible mechanism for the phenomenon of reactive inhibition. Repetitive stimulation would have the effect of progressively increasing the inhibitory state (hyperpolarization). If continued stimulation had a differential effect, such that it increased inhibitory state more than or faster than it increased excitatory state, we would have a simple and accurate mechanism of reactive inhibition at the neurophysiological level. Hyperpolarization followed by "disuse" might be suggested as the neural paradigm of spontaneous recovery. We obviously need more neurophysiological experiments on inhibitory effects.

The history of science has shown again and again that development of an important new method is followed by crucial and often epoch-making discoveries. There are indications that Eccles' new micro-electrode technique is already yielding such discoveries. This, rather than theoretical considerations, is what I would stress in calling attention to his book. Yet we may hope that future work will prove what now appears to be the case—that the method is capable of revealing the neural mechanisms underlying the phenomena of conditioning and learning. With the new information already provided it appears possible to rule out certain factors in some earlier models of the conditioned reflex. In working out his own model, Eccles was careful not to violate neurophysiological fact in any particular, and was able to sketch in some new features, documented with very recent experimental data. One part of his model, while in keeping with neurophysiological fact, did not appear to conform to what is known about behaviour. We have ventured to suggest a possible correction, based on his own experiments. Eccles is working close to the borderland between psychology and neurophysiology, where co-operative efforts across fields may bring science closer to elusive goals.

REFERENCES

1. Brock, L. G., Coombs, J. S., & Eccles, J. C. The recording of potentials from motoneurones with an intracellular electrode" *J. Physiol.*, 1952, **117**, 431–460.
2. Eccles, J. C. *The neurophysiological basis of mind. The principles of neurophysiology.* Oxford: Clarendon Press, 1953.
3. Hebb, D. O. *The organization of behavior.* New York: Wiley, 1949.
4. Hilgard, E. R., & Marquis, D. G. *Conditioning and learning.* New York: Appleton-Century, 1940.
5. Hull, C. L. *Principles of behavior.* New York: Appleton-Century, 1943.

THE EFFECT OF STIMULUS POSITION ON VISUAL DISCRIMINATION BY THE RAT[1]

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PATTERN vision in the rat has been most effectively studied by a jumping method devised by Lashley (4). This procedure requires the rat to jump at one of two cards bearing the patterns to be discriminated. On the basis of Lashley's extensive anatomical and behavioural studies it has been assumed that the cards fall within the rat's binocular field of vision and hence are seen as a whole at the time when the visual patterns begin to influence behaviour during discrimination learning. Ehrenfreund (2), however, thought that the rat's effective field of vision in the Lashley jumping apparatus might be limited to the lower section of the cards, and has demonstrated this experimentally in certain conditions of training.

The question may be raised, however, whether Ehrenfreund's results demonstrate that the animal's field of vision is small, or simply that it fails to attend to the upper part of its visual field, at least during the early stages of learning. This is an important question because of the place of attention in the theory of behaviour. Stimulus-response psychologists have found difficulty with the concept of attention, ignoring it or attempting to show that it is unnecessary, and Ehrenfreund's experiment has been regarded as supporting their point of view. The purpose of the present paper is to show that the rat's failure to respond to the upper parts of the stimulus card in Lashley's procedure is due to "inattention" rather than inability to see.

Ehrenfreund trained his animals under two conditions. The stimulus patterns were an erect and an inverted triangle. In one condition the stimuli were above the level of the animal's eyes; in the other, the platform from which the animal jumped was raised level with the stimuli. In the first condition, learning was retarded; Ehrenfreund concluded, therefore, that in the usual experimental conditions only the bottom of the card is responded to, and that during the early stages of training the rat does not see the upper part.

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A method of testing this conclusion is available. Discrimination of horizontal from vertical lines is easier for the rat than discrimination of erect from inverted triangles, requiring about one-third the number of trials for complete learning. It is possible, then, to give the rat a combination of stimuli, the "easier" striations in the upper half of the field, and the "harder" triangles below. If he sees the upper half with difficulty, or does not see it, the rate of learning should be slower than when striations are below and the triangles above. If, on the other hand, the rat sees both fields equally well, he will be able from the first to respond to the easier pattern, the striations, whether they are above or below. The hypothesis on which this experiment was based, therefore, is that the rate of learning will be equally rapid whether the striations are in the lower or the upper half of the field. This would mean that under Ehrenfreund's experimental conditions the rat *attends* more to the lower part of the field; not that he has difficulty in seeing the whole field.

METHOD

Apparatus

The rats, 60 hooded males between three and five months of age, were trained on a modified Lashley jumping apparatus. The openings leading to the feeding platform were 14.5 cm. by 12.5 cm., 4 cm. apart. The jumping platform was at a distance of 20.5 cm. from the feeding platform and approximately level with it.

Pre-training Procedure

The rats were trained to jump for food, at first to a plain white card in preference to a plain black card. This took from two to ten days. If on any trial the rat had not jumped after about five minutes, the tail was tapped in the usual way. Ten trials a day were given. Pre-training was completed when 20 errorless jumps were made. The cards to be discriminated were alternated left and right, in random order (the Gellerman series). The non-correction method was used, i.e., the rats made a total of 10 jumps on any one day without being able to correct their mistakes. This differs from the correction method, wherein the rat is allowed to jump to the wrong pattern until he either chooses the correct one himself, or is forced to do so by the experimenter. The correction method results in massed practice, and presumably affects the way in which the animal learns the problem.

Training Procedure

Group I consisted of 21 rats which were presented with a more difficult pattern at the bottom of the cards and an easier pattern at the

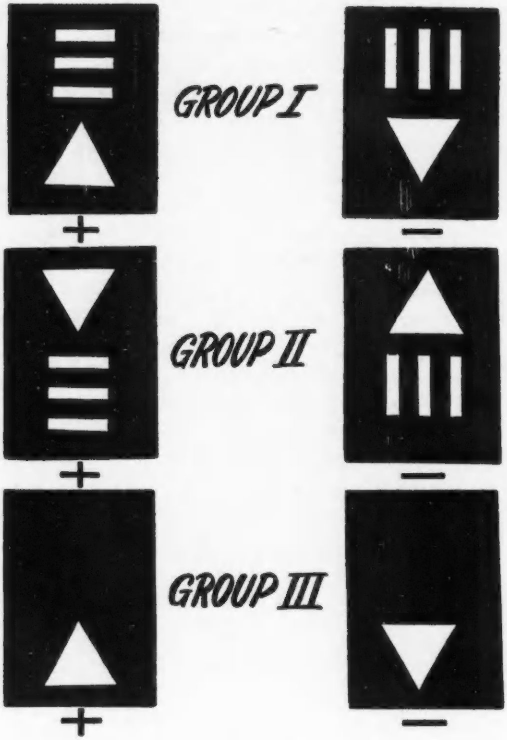

FIGURE 1

top; the lower pattern was a white triangle, the upper consisted of three white striations (Fig. 1). The equilateral triangle had sides of 6 cm., and the striations were 5 by 1 cm. each, 1.5 cm. apart. Striations and triangles were matched for total area of white. The positive pattern consisted of an upright triangle and horizontal striations; the negative of an inverted triangle and vertical striations.

Group II contained 18 rats which were presented with the easier pattern (striations) at the bottom of the cards and the more difficult one (triangle) at the top (Figure 1). Here horizontal striations were paired with an inverted triangle and vertical striations with an upright one. Apart from their position, triangles and striations were identical with those presented to Group I.

Group III consisted of 10 rats which were presented with the hard pattern alone: erect and inverted triangles, located at the bottom part of the cards (Figure 1). The triangles were the same as for Groups I and II.

Group IV consisted of 11 rats which were presented with the easy pattern alone. Six rats were trained to discriminate striations in the upper half of the cards, five to discriminate striations at the bottom of the cards.

Two successive days of at least 18 correct choices out of 20 trials constituted the criterion of completed learning. As soon as each rat of the first two groups had met criterion it was tested for transfer of training, with the upper halves of the stimulus patterns alone. On these trials a choice of either card was rewarded. Next day, after 10 further trials with the original (complete) patterns, the rat was presented with the lower halves. The same order of presentation (upper halves and then lower halves) was followed for both groups.

Observations were also made of the time taken to jump by eight rats which, during pre-training and the first day of training, jumped readily. The latency—the time taken to jump—was recorded up to and including the first day on which the rat met the criterion of learning. The rat develops a position habit when first presented with a new problem, jumping always to a preferred side rather than to a particular pattern. The latency records made it possible to compare the average delay before the rats jumped to (*a*) the correct, and (*b*) the wrong pattern on their preferred side, before and just after the position habit was abandoned.

RESULTS

The results of the first part of the experiment can be seen in Table I. They show that the complex pattern containing both triangles and striations was significantly easier than the one with triangles alone.

TABLE I

LEARNING SCORES FOR GROUPS I, II, AND III

Group	N	Trials			Errors	
		Mean	*s*	Range	Mean	Range
I	21	70.0	26.5	30–120	32.6	8–62
II	18	67.7	28.0	20–120	32.7	4–52
III	10	159.0	57.53	20–210*	77.5	9–106

*With the exception of one animal which learned the problem in 20 trials and made 9 errors, the trial range for Group III is 110–210 and the error range 52–106.

The trial and error scores of Groups I and II are less than half those of Group III.

It is also apparent that the rate of learning the complex pattern was about the same with each of the two arrangements: striations above triangles (Group I), and striations below triangles (Group II). Groups I and II took approximately the same number of trials (the group means being 67.7 and 70.0 trials), and made the same number of errors (32.6 and 32.7). The standard deviations of the trial scores for the two groups were 26.5 and 28.0, respectively.

An F ratio of 23.6 indicates that the difference between some of the means for trial scores is significant at the .001 level of confidence. Analysis of variance and the *t*-technique were used in spite of the inequality of variances. The significantly larger variance of Group III can be accounted for by the score of one animal, which took only 20 trials to learn the problem. This animal's score does not bias the results in favour of the original hypothesis, and makes both the *F* and *t* ratios lower than they would have been otherwise.

The results of training two groups, of six and five rats, to striations at the top and bottom of the cards, respectively, show no significant differences in trial or error scores. It also appears that the pattern containing striations alone is not significantly easier than the complex patterns containing both striations and triangles. (The results may not be fully comparable with those obtained for the first three groups, since the 11 rats in Group IV were trained at a later period with a possible change of conditions. However, the comparison of rates of learning by the two halves of Group IV is valid.)

Transfer Trials

The results obtained for Groups I and II during transfer trials to the top and bottom parts of the cards are presented in Table II. Each group was tested by the omission of the upper or lower patterns. Thus Group I, trained with striations above and triangles below, was first tested

TABLE II

NUMBER OF RATS TRANSFERRING TO TOP OR BOTTOM HALF OF PATTERN, TO BOTH, AND TO NEITHER

(Group I, "easy" pattern above; Group II, "easy" pattern below)

Group	N	Top	Bottom	Both	Neither
I	21	14	10	7	4
II	18	4	18	4	0
Total	39	18	28	11	4

with striations alone, still in the upper part of the cards, and then with triangles alone, in the lower part. Ten choices out of 10 trials of the part of the diagram originally positive constituted the criterion of transfer. It is apparent from Table II that significantly more animals transferred to the top of the cards when striations were there than when the more difficult pattern (triangles) was there (14 out of 21 rats vs. 4 out of 18 rats). The significance of the difference, by the X^2 method, is at the .01 level of confidence.

Significantly more rats transferred to striations when they were at the bottom of the cards than when they were on top (18 vs. 14, $P < .01$); significantly more transfers were made to the bottom of the card when it contained the striations than when it contained the more difficult triangles (18 vs. 10, $P < .01$); and significantly more rats transferred only to the bottom of the card when striations were there than when it contained triangles (14 out of 18 rats vs. 6 out of 21, $P < .01$).

From these results it is clear that some animals perceived, or attended to, the whole figure; others, one part only. Eleven rats of 39 in Groups I and II transferred to each component when presented alone; four rats were greatly disturbed and performed at a chance level when either component was missing during transfer trials. We may say, therefore, that the latter four animals perceived the pattern as a whole in learning; 11 perceived the whole pattern, but also perceived both halves as independent entities and could respond on the basis of either; and the remaining 24 rats in Groups I and II learned to respond on the basis of perceiving only one or the other half of the pattern.

Finally, the latencies of jumps to correct and incorrect cards on the preferred side were computed. The mean latency per trial (for 8 rats in 330 trials) to the correct pattern was 0.59 min., whereas that to the incorrect pattern was 2.00 min. The difference of 1.41 min., as indicated by a t ratio of 4.2, is significant at the .01 level of confidence. The formula applied was that suggested by McNemar (8) for comparing correlated means.

DISCUSSION

The results obtained in the present experiment only partly confirm those of Ehrenfreund (2), and throw additional light on the nature of pattern discrimination in the rat as it occurs in the Lashley jumping apparatus. The present discussion will concern itself with two topics: (1) the relative importance for pattern discrimination of the bottom and top of the stimulus cards in relation to the nature of the patterns used; and (2) the implications of the present results for the continuity-noncontinuity controversy regarding the nature of learning.

1. If, as Ehrenfreund asserts (2), the bottom of the card is of most

importance in learning the discrimination, then placing the "easier" pattern at the bottom should produce quicker learning. This was not so, however, as Groups I and II had similar learning rates. On the other hand, the results of the transfer trials to the upper and lower halves of the cards (Groups I and II) appear to support Ehrenfreund's position, a higher proportion of rats transferring to the lower halves of the diagrams (18 vs. 14, $P < .01$; and 10 vs. 4, $P = .10$).

However, further consideration reveals that the relative importance of the bottom and top of the cards depends partly on the nature of the patterns. Thus the bottom of the cards yielded fewer errorless choices during critical trials when it contained a difficult pattern (10 vs. 18, $P < .01$; see Table II). And when the upper half of the cards contained an easy pattern, it was more effective in eliciting transfers than when it contained a difficult one (14 vs. 4, $P < .01$).

The fact that there was no significant difference in the learning rates of Groups I and II means that no additional time was spent by rats in Group I in learning to see the easier component (whose presence determined the rate of learning of both groups), even though this component was high on the cards; it is justifiable to conclude, therefore, that the stimulus cards were seen as a whole from the time the presence of visual patterns began to influence the response.

This conclusion is further substantiated by the fact that significant differences in latencies of jumps occurred at the same stage of learning for both types of patterns. The rats in groups I and II took significantly more time to jump to the incorrect pattern than to the correct, when each was presented on the preferred side. This indicates that the rats began to discriminate the pattern with the easy component at the top as early as they did that with the easy component at the bottom.

These results do not confirm Ehrenfreund's conclusion that only the bottom of the card determines the response during the early stages of learning.

2. The second question concerns the relation of the present results to recent discussions regarding the nature of learning. These have been extensively reviewed elsewhere (**1**, **2**, **3**, **10**). Learning, according to insight theorists, proceeds by way of organized attempts at solution. This may produce sudden improvements in learning (hence the recent use of "noncontinuity" for what was earlier called "insight"). The "continuity" position, as conceived by Spence (**10**), is that learning is a gradual process of building up associative tendencies. This system does not explain discrimination behaviour in terms of perceptual organizations, or by trying to understand the related central process of attention. In order to preserve the stimulus-response position, the attempt is made

to identify attention with adequacy of peripheral stimulation at the time of response. Ehrenfreund's experiment represents such an attempt; the present experiment makes it look less convincing than at first appeared.

On the other hand, the findings on latency of jumps in the present experiment require some modification of the position taken by the proponents of insightful learning. Following Krechevsky (3), it is usually assumed that one "hypothesis" or perceptual set is operative at any one time. The animals of the present experiment showed, by jumping more quickly to correct than to incorrect patterns (even while still jumping always to one side), that their behaviour was under the control of *two* hypotheses at the same time. The visual hypothesis did not spring into existence suddenly, at the moment when the spatial hypothesis was extinguished, but was built up gradually, with continuity in its development. It must be recognized, therefore, that perceptual sets, which eventually produce discontinuity in the learning process, need not be an all-or-none process. The evidence supplied by Blum and Blum (1), together with the present data, makes this conclusion inevitable.

Summary

Ehrenfreund has demonstrated under certain experimental conditions that in the Lashley jumping apparatus only the bottom of the cards is seen at the early stages of learning. Further investigation of pattern-discrimination learning in the rat, using easy and difficult patterns, only partly confirmed Ehrenfreund's results. Whereas under certain conditions the bottom of the card is more readily learned than the top, this relative importance is a function of the difficulty of the patterns to be perceived. The relevance of these results to Ehrenfreund's criticism of the noncontinuity theory of learning is briefly discussed.

REFERENCES

1. Blum, R. A. & Blum, Josephine S. Factual issues in the "continuity" controversy. *Psychol. Rev.*, 1949, **56**, 33–50.
2. Ehrenfreund D. An experimental test of the continuity theory of discrimination learning with pattern vision. *J. comp. physiol. Psychol.*, 1948, **41**, 408–422.
3. Krechevsky, I. A study of the continuity of the problem-solving process. *Psychol. Rev.*, 1938, **45**, 107–133.
4. Lashley, K. S. The mechanism of vision. I. A method for rapid analysis of pattern-vision in the rat. *J. genet. Psychol.*, 1930, **37**, 353–460.
5. Lawrence, D. H. Acquired distinctiveness of cues: I. Transfer between discriminations on the basis of familiarity with the stimulus. *J. exp. Psychol.*, 1949, **39**, 770–784.
6. Lawrence, D. H. Acquired distinctiveness of cues: II. Selective association in a constant stimulus situation. *J. exp. Psychol.*, 1950, **40**, 175–188.

7. Lawrence, D. H. The transfer of a discrimination along a continuum. *J. comp. physiol. Psychol.*, 1952, **45**, 511–516.
8. McNemar, Q. *Psychological statistics*. New York: Wiley, 1949.
9. Meehl, P. E., & MacCorquodale, K. Some methodological comments concerning expectancy theory. *Psychol. Rev.*, 1951, **58**, 230–233.
10. Spence, K. W. An experimental test of the continuity and non-continuity theories of discrimination learning. *J. exp. Psychol.*, 1945, **35**, 255–266.

RELIABILITY OF THE CLOSED-FIELD TEST FOR RATS ADAPTED FOR WATER-ESCAPE MOTIVATION[1]

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ROSVOLD and Mirsky (7) demonstrated that the reliability of the closed-field intelligence test for rats as adapted for water-escape motivation[2] may be low if the rats have not received considerable practice in the test. However, the low reliability may have been a function of the method used (interpolation of the floor test between test and retest on the water test) rather than of insufficient practice. The first experiment to be reported here determines the reliability of the water test over three successive repetitions without interpolated floor tests, and demonstrates the effect of practice on the rat's error and time scores.

The first paper (7) made two assumptions: (1) the water test would be useful in discriminating electroshocked from normal rats; (2) increased motivation for food would not improve the rat's performance in the water test. The second experiment reported here tests these assumptions.

PROCEDURES

Twenty-four Sprague-Dawley albino rats, 120 days old at the beginning of the study, were maintained in individual cages on Purina Laboratory Chow and water, both *ad libitum*. For 14 days they were tamed by gentle handling and stroking. They were then given the preliminary training as described by Rosvold and Mirsky (7). Starting the day after reaching criterion on the preliminary training, they were tested on one problem per day until they had been tested on all 12 problems of the test. This series is designated *Test*. After a two-day rest, they were tested on the same 12 problems one per day. This series is designated *Retest 1*. After another two-day rest they were tested similarly on the 12 problems a third time. This series is designated *Retest 2*. Finally, aften an interval of 14 days, they were tested on the same 12 problems

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[2] Hereafter called the water test.

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a fourth time. This series is designated *Retest 3* and is the series used in Experiment 2. Both time and error scores were recorded.

A test trial was terminated when any one of the following conditions was met: (1) both forepaws touched the exit ramp; (2) the animal remained in the water for 300 seconds; or (3) made 30 errors; or (4) sank. If the animal had not reached the exit ramp at the termination of the trial, it was guided to it by the experimenter before being removed from the water. The trial following removal under conditions (2), (3), or (4) was terminated after 10 seconds of swimming, and the third unsuccessful trial in a row after 60 seconds. No further trials were given on that day. The score for such a day was the total errors and time, regardlesss of number of trials.

The apparatus used was described in an earlier study (7). The experimental room was not lighted artificially, and illumination was maintained at a constant level by means of dark window shades. The water in the maze was maintained at a depth of 9½ inches and was regulated to be within one degree of 27 degrees Centigrade.

For Experiment II the animals were divided into four equal groups matched on the basis of their total time scores in Retest 2. The resulting groups did not differ significantly from each other on total error scores. Two groups (ST and SHST) were food-deprived by starving them for 72 hours, then feeding them for 10 days on Purina Laboratory Chow pellets, one hour per day. The other two groups (NO and SH) were continued on Purina Laboratory Chow *ad libitum.* During these 10 days, Groups SHST and SH were administered one electroconvulsive shock per day as described by Mirsky and Rosvold (4); Groups ST and NO were handled, but not shocked. On the day following the tenth shock, Retest 3 was begun.

RESULTS OF EXPERIMENT I

Error Scores

As shown in Table I, each time the animals were administered the 12 problems, the mean total error scores decreased significantly. It is evident that such a decrease was obtained even upon a third retest. Therefore, if a decrease in error score should not occur in a retest, after treatment had been interpolated between test and retest, a deleterious effect may be confidently attributed to the treatment. However, it is to be noted that the rank order correlation (rho) of total error scores between Test and Retest 1 is .23; between Retest 1 and Retest 2 it is .78. This would suggest that the error score obtained from the water adaptation of the Hebb-Williams test may not be a reliable measure until the rats have had considerable practice. It appears that the preliminary practice on problems A to F was not sufficient to ensure a reliable performance on the early problems of Test.

Time Scores

As shown in Table II, a significant decrease from the immediately preceding time scores exists only between Test and Retest 1. Thus a deleterious effect may be confidently attributed to a treatment if, when interposed between Test and the first retest, it fails to result in a decrease in time scores of a retest; or if it results in an increase in time scores when the treatment is interposed between later retests.

In contrast to the total error score, the total time score is a reliable measure after any level of practice. The rank order correlation of mean time scores between Test and Retest 1 is .75; between Retest 1 and Retest 2 is .73; and between Retest 2 and Retest 3 is .99.

Error and Time-Score Comparisons

Table III shows the rank order correlation between total error and

TABLE I

MEAN TOTAL ERROR SCORES FOR EACH TEST SERIES

	Test series							
	Test		Retest 1		Retest 2	Retest 2*		Retest 3*
N	24		24		24	6		6
Mean	160.3		69.6		47.7	54.8		34.3
Mean diff.		90.7		21.9			20.5	
σ Mean diff.		8.7		2.9			5.5	
t		10.4		7.6			3.7	
p		$<.001$		$<.001$			$<.01$	

*Based on only the six untreated animals of Experiment II.

TABLE II

MEAN TOTAL TIME SCORES (SECS.) FOR EACH TEST SERIES

	Test series							
	Test		Retest 1		Retest 2	Retest 2*		Retest 3*
N	24		24		24	6		6
Mean	1662.3		911.5		863.4	828.9		780.1
Mean diff.		750.8		48.1			65.6	
σ Mean diff.		59.9		39.7			42.0	
t		12.6		1.2			1.56	
p		$<.001$		$>.10$			$>.10$	

*Based on only the six untreated animals of Experiment II.

total time scores for each of the test series. It is clear that although each rho is significant, a prediction from one score to another cannot be made with much confidence.

It is evident from Table IV that, whereas the rank order correlations between time scores on individual problems and the total time scores for the set of 12 problems of this series are all high, only some of the error scores on individual problems correlate highly with the total error score. The speed with which an animal swims on any one problem is, therefore, likely to be a reliable estimate of its overall time score; only on some of the problems is the accuracy of its performance likely to be a reliable estimate of overall error score. This would suggest that the test could be improved by dropping the inconsistent items or by developing consistent ones. However, it is likely that an animal's performance on one problem is not independent of the preceding ones, and therefore the relationships found to exist for this series of 12 problems may not exist if any one problem is changed.

RESULTS OF EXPERIMENT II

Experiment II was designed (1) to test the usefulness of the water test in discriminating between electroconvulsed and untreated animals, and (2) to demonstrate the effect of an "irrelevant drive" (i.e. increased food

TABLE III

RANK ORDER CORRELATION BETWEEN TOTAL TIME AND TOTAL ERROR SCORES FOR EACH TEST SERIES

	Test series		
	Test	Retest 1	Retest 2
Rho between time and error scores	.48	.46	.50
p	$<.05$	$<.05$	$<.05$

TABLE IV

RANK ORDER CORRELATION BETWEEN THE SCORE ON EACH PROBLEM AND THE TOTAL SCORE OF RETEST 2

	Problems											
	1	2	3	4	5	6	7	8	9	10	11	12
Time	.72	.81	.78	.70	.88	.84	.80	.84	.73	.86	.72	.82
Errors	.41	.72	.29	.51	.77	.15	.67	.70	.40	.60	.77	.51

deprivation) on performance in the water test. The 24 animals of Retest 2 were divided into four groups equated on the basis of their time scores. One group (ST) was food deprived; another (SH) was electroconvulsed; another (SHST) was convulsed and deprived; the other (NO) neither convulsed nor deprived. One animal in the SHST group died during the experiment. For purposes of statistical analysis of the data his probable score was estimated by Yates's method (1, p. 110).

Error Scores

Experiment I indicated that a deleterious effect may be attributed to a treatment if upon retest there is no decrease in error score. The proposition that for each group there is no decrease in total error scores between Retest 2 and Retest 3 (the treatments having been interposed), may be tested by Wilcoxon's Matched Pairs Signed Ranks Test (5). This proposition can be rejected with confidence ($p = .05$) only for the NO group. The effect on convulsions, food deprivation, or both was, therefore, to prevent a decrease in the total error score; that is, a deleterious effect.

Applying the Mann-Whitney U Test (3) to the differences between groups in the decrease in total error scores indicates, as shown in Table V, that the convulsed group, the food-deprived group, and the group both convulsed and deprived differ significantly from the group receiving no treatment. The treated groups do not differ among themselves. An analysis of these differences using Fisher's t test gives the same results.

It has been shown that with respect to error decrease each of the groups of treated animals differs from the untreated group. However, if a treated group is considered together with the untreated to be an N of 12, having been drawn from the same population, and if it is expected that a treated rat will have no decrease in error score while an untreated rat will have, a sign test (2) may be applied to the instances of agree-

TABLE V

PROBABILITY THAT GROUPS DIFFER IN DECREASE IN TOTAL ERROR SCORES

Groups compared	Probability that samples differ
SHST and ST	$p > .05$
SHST and SH	$p > .05$
ST and SH	$p > .05$
SH and NO	$p < .03$
ST and NO	$p < .03$
SHST and NO	$p < .04$

ment of each animal's performance with his expected performance. Such an analysis indicates that for an SH and SHST animal, but not an ST animal, the agreement is significantly better than chance ($p = .01$, .01, and .25, respectively). Thus the water test, in addition to discriminating the treated groups from the untreated group on the basis of a decrease in error scores, discriminates from an untreated animal one which has been shocked, or shocked and deprived. It does not discriminate one which has been food-deprived only.

Time Scores

The proposition that for each group there is no difference in total time scores between Retest 2 and Retest 3 (the treatments having been interposed) may be tested by Wilcoxon's Matched Pairs Signed Ranks Test (5). This Proposition cannot be rejected with confidence ($p > .05$) for any of the groups. Thus, there is no effect on total time scores of the convulsions, of the food deprivation, or the convulsions together with deprivation. Experiment I indicates that such an effect occurs in the absence of treatment.

Similarly an analysis by the Mann-Whitney U Test (3) of the differences in time scores between any one treatment and another, or between any one treatment and no treatment, indicates that none of the differences is significant ($p > .05$). An analysis of these results using Fisher's t gives the same results.

It has been shown that with respect to time scores none of the treated groups differs from the untreated group. However, if a treated group is considered together with the untreated group to be an N of 12, having been drawn from the same population, and if it is expected that the treated rat will have an increase in time scores while the untreated will not, a sign test (2) may be applied to the instances of agreement of each animal's performance with his expected performance. Such an analysis indicates that for each animal the agreement is probably not better than chance ($p >. 25$). Thus the water test does not, on the basis of time scores, differentiate reliably a treated from an untreated animal.

DISCUSSION

Experiment I set out to demonstrate the reliability of the water test over successive repetitions, and the effect of practice on the rat's error and time scores. It is evident that the time scores achieved on the first administration of the 12 problems are a reliable indication of the time scores likely to be achieved upon a second administration. It is, however, the error score achieved during a second repetition of the 12 problems which is a reliable indication of error scores likely to be achieved upon further repetitions. This would indicate that even when the floor test is not interpolated between repetitions of the water test (as in (7)) more practice is required on the water than on the floor test in order to achieve comparable reliability. The effect of practice on error scores is evident even after three repetitions of the test; the effect on time scores is not apparent after the first repetition.

Experiment II set out to demonstrate the validity of the assumptions (1) that the water test would discriminate electroconvulsed rats from normal rats, and (2) that increased motivation for food would not improve the rat's performance in the water test. It is evident that electroconvulsed rats can, as individuals or as groups, be distinguished from normal rats on the basis of decrease in error scores; the shocked rats fail to improve performance while normal rats do improve. Electroshocked rats cannot, as a group, or as individuals, be distinguished reliably from normal rats on the basis of time scores. Perhaps the effect of shock would have been reflected in the time scores had the rats been shocked earlier, i.e. after Test.

It is evident, too, that the increased motivation for food (increased food deprivation) does not improve the rat's performance in the water test, thus justifying the assumption in the first paper (7). However, the rat's performance in the water test is not, as was implied, unaffected by motivation for food. It is, in fact, sufficiently impaired to make the food-deprived group indistinguishable from the electroconvulsed group with respect to error decrease. Thus, increasing the irrelevant drive for food disturbs the rat's performance on the water test. A similar effect was noted by Mueninger and Fletcher (6) who found that hungry rats made more errors than satiated rats in an escape-from-shock situation. Whether this effect is true of other irrelevant drives cannot be determined from this study. However, it is clear that the performance of a rat in an escape situation is disturbed by increasing motivation for food. Therefore, it is not appropriate to use the water test as a measure of the effect of a treatment on intelligence, unless it can first be shown that the treatment does not affect motivation for food. Since increased motivation for food improves performance on the floor test but impairs performance on the water test, the two tests used together may enable the experimenter to separate the effect of a treatment on intelligence from its effect on food motivation. A relatively equal impairment in performance in both forms of the test would suggest that the effect of a treatment like electroshock on test performance (i.e. intelligence) is independent of its effect on motivation for food.

The low reliability in error scores upon a first repetition of the test suggests that a change should be made in the preliminary training trials.

Examination of individual rats' early behaviour relative to its effect on rank orders suggests the following changes: preliminary swimming trials (without barriers) are unnecessary; each animal should receive nine trials per day on the practice problems until the series A to F has been completed twice; any animal who exceeds a total swimming time of 450 seconds on the second repetition of problems A to F should be discarded; if by problem F of the second repetition an animal still shows fright upon being removed from the ramp, he should be discarded; the experimenter should expect to discard about 20 per cent of his animals at this stage.

SUMMARY

1. The procedure for use of the water test has been described.

2. The water test has been shown to be reliable with respect to both time and error score measures, though considerable practice is required to achieve reliability in the error score measure.

3. The error score on the water test discriminates electroconvulsed and food-deprived rats from untreated rats. The effect of shock and of food deprivation is to preclude further decrease in errors.

4. The time score, at the level of training used in this study, does not discriminate electroconvulsed or food-deprived rats from untreated rats.

5. The effect of increased food deprivation is to impair performance. Modifications in the use of the test are discussed.

REFERENCES

1. Cochran, W. G. & Cox, G. M. *Experimental designs.* New York: Wiley, 1950.
2. Dixon, W. J., & Mood, A. M. The statistical sign test. *J. Amer. statist. Ass.*, 1946, 41, 557–566.
3. Mann, H. B. & Whitney, D. R. On a test of whether one of two random variables is stochastically larger than the other. *Ann. math. Statist.*, 1947, 18, 50–60.
4. Mirsky, A. F., & Rosvold, H. E. The effect of electroconvulsive shock on food intake and hunger drive in the rat. *J. comp. physiol. Psychol.*, 1953, 46, 153–157.
5. Moses, L. E. Non-parametric statistics for psychological research. *Psychol. Bull.*, 1952, 49, 122–143.
6. Muenzinger, K. F. & Fletcher, J. Motivation in learning. *J. comp. Psychol.*, 1936, 22, 79–91.
7. Rosvold, H. E., & Mirsky, A. F. The closed-field intelligence test for rats adapted for water-escape motivation. *Canad. J. Psychol.*, 1954, 8, 10–16.

GENTLING AND WEIGHT GAIN IN THE ALBINO RAT

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In recent years there has been much interest in the effect of early handling on the later behaviour of the organism. The early studies of Greenman and Duhring (4) indicate that handling and petting are essential for the best growth of albino rats, and that such gentled rats withstand surgical experience (parathyriodectomy for example) more adequately than ungentled rats.

Experiments performed by Weininger (7, 8) have shown that albino rats, gentled during infancy, consistently gained more weight than did comparable non-gentled ones. The results of these experiments indicate that the increased weight is due, not only to an increased deposition of adipose tissue, but also to an increased skeletal length. In general, the gentled animals simply grew more. Thus, weight gain appears to be one of the variables dependent on early handling. Further experiments performed by Weininger (9) and by Bernstein (2) appear to substantiate these preliminary findings.

Experiments conducted by Vetulani (as reported by Allee (1)) have shown that albino mice living in groups are heavier than mice individually housed. He found that mice isolated in separate cages failed to grow as fast as those that were grouped two, four, or six in a cage. However, the experiments of Weininger (7, 8, 9) and Bernstein (2) suggest that gentling may be more beneficial than group-living, even when the gentled animals are caged individually. Therefore, the present study compares the amount of weight gained by three groups of rats: gentled, non-gentled, and group-living.

The hypotheses investigated were: (*a*) that albino rats, gentled for 10 minutes a day for three weeks after weaning would show a significantly greater mean weight than comparable non-gentled and group-living rats; and (*b*) that the group-living rats would show a significantly greater mean weight than the non-gentled ones.

PROCEDURE

Thirty male albino rats, Wistar strain, were obtained from the Carworth Farms, New York, and were sorted at random into three groups

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of 10 animals each (using the Table of Random Numbers (6)). This sorting was done at the age of 21 days, immediately after weaning. Two of the groups were housed in individual 12″ x 12″ x 12″ metal cages, while the third group was housed in a 36″ x 24″ x 18″ wire-mesh common cage. The mean weight of each group, at this time, was approximately the same, 38 grams.

The 10 animals in the experimental group were gentled for 10 minutes a day for 21 days. Gentling consisted of holding the animal in the experimenter's left hand with the hand placed against the experimenter's chest. The right thumb stroked the back of the animal from the head to the base of the tail, at the rate of approximately 50 strokes a minute. The animal was not held tightly and was allowed to move about in the experimenter's hand.

During this time, the animals in the non-gentled and group-living groups were not handled by the experimenter; but in all other respects they were treated exactly like the experimental group. Each animal received an *ad libitum* amount of Purina Fox Bar food cubes and water; in addition, the animals were given small pieces of green vegetables once a week. The temperature of the laboratory was held constant at 74°F.

All animals were weighed at 42 days of age, and at three-day intervals thereafter.

RESULTS

The initial hypothesis of this study, that albino rats gentled for three weeks after weaning would show a significantly greater mean weight than comparable non-gentled or group-living animals, is supported by the data. Tables I and II show that at the age of 21 days there was no significant difference between the mean weights of the three groups, but that at 42 days of age, the gentled rats were significantly heavier than

TABLE I

MEAN WEIGHT OF GENTLED AND NON-GENTLED MALE ALBINO RATS

Age	Mean weight (gentled)	Mean weight (non-gentled)	S.E. of difference	t	P
days	*gm.*	*gm.*			
21	38.8	38.4	1.90	0.21	$>.50$
42	145.1	122.5	5.74	3.94	$<.01$
45	162.3	139.0	6.79	3.43	$<.01$
48	177.6	152.3	7.30	3.47	$<.01$
51	195.4	167.8	7.14	3.87	$<.01$
54	209.7	177.1	9.69	3.36	$<.01$

either the non-gentled or group-living animals ($P < 0.01$). The second hypothesis, however, was not confirmed: Table III indicates that there was no significant difference in the mean weight of the non-gentled and group-living animals within the age range of the experiment. The differences are illustrated in Figure 1.

TABLE II

MEAN WEIGHT OF GENTLED AND GROUP-LIVING MALE ALBINO RATS

Age	Mean weight (gentled)	Mean weight (group-living)	S.E. of difference	*t*	P
days	*gm.*	*gm.*			
21	38.8	38.6	1.30	0.15	$>.50$
42	145.1	120.2	6.26	3.97	$<.01$
45	162.3	138.9	7.04	3.32	$<.01$
48	177.6	151.8	7.53	3.43	$<.01$
51	195.4	168.7	7.39	3.61	$<.01$
54	209.7	176.6	9.44	3.51	$<.01$

An analysis of the amount of weight gained by the gentled, non-gentled, and group-living animals, between the ages of 21 days and 42 days, and between 21 days and 54 days, showed that the gentled animals gained significantly more weight during both periods than did either of the other two groups ($P < 0.001$). There was no difference between the gains of the non-gentled and group-living rats during these periods. Thus, the gentled animals not only weighed more than the non-gentled or group-living animals at any given period, but also gained more weight from one weighing period to the next.

TABLE III

MEAN WEIGHT OF NON-GENTLED AND GROUP-LIVING MALE ALBINO RATS

Age	Mean weight (non-gentled)	Mean weight (group-living)	S.E. of difference	*t*	P
days	*gm.*	*gm.*			
21	38.4	38.6	1.50	0.13	$>.50$
42	122.5	120.2	5.06	0.45	$>.50$
45	139.0	138.9	6.58	0.02	$>.50$
48	152.3	151.8	7.01	0.07	$>.50$
51	167.8	168.7	7.37	0.12	$>.50$
54	177.1	176.6	8.06	0.06	$>.50$

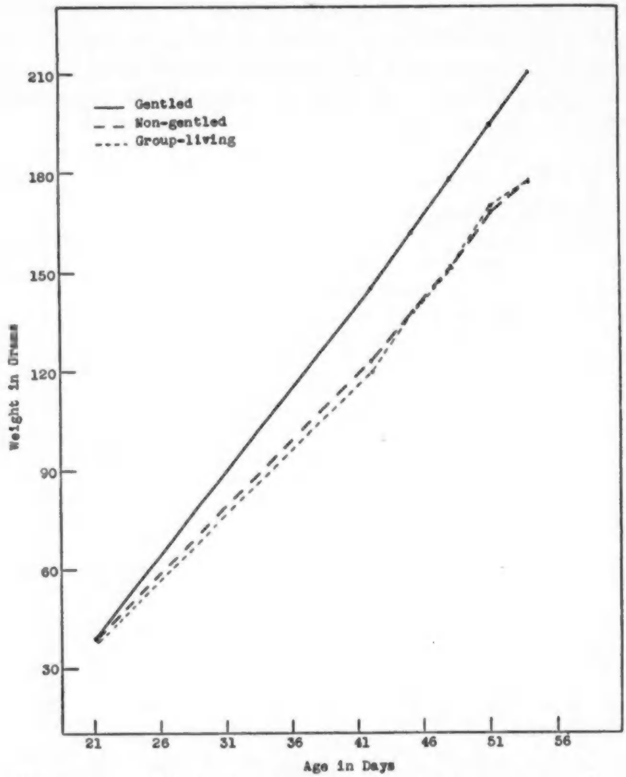

FIGURE 1. Average weight of rats in gentled, non-gentled, and group-living groups.

DISCUSSION

The results of the present study show that gentled albino rats gain more weight on an *ad libitum* feeding diet than do non-gentled or group-living animals. Further, contrary to the second hypothesis, there was no significant difference between the mean weights of the individually caged (non-gentled) and the group-living animals. These findings are contrary to the results observed by Vetulani (**1**) in that gentled animals, when housed individually, gained more weight than did group-living animals, while group-living and individually housed animals weighed the same.

Since gentling is the determining variable, the crucial factor affecting the growth rate may be a change in the activity of the sympathetic nervous system in the gentled animals. Gentling, operating as tactile stimulation to proprioceptive receptors on the skin, may affect the thalamus (3). The direct interconnections of the thalamus with the hypothalamus put the former in a position to affect the autonomic and endocrine functions of the organism. This change may be in the direction of raising the threshold for sympathetic discharge, with an ensuing decrease in this activity. Such a reduction in sympathetic activity would have the essential effect of increasing anabolic activity. Therefore gentled animals would weigh more.

REFERENCES

1. Allee, W. C. *The Social Life of Animals.* New York: Norton, 1938.
2. Bernstein, L. A note on Christie's "Experimental naivete and experiential naivete." *Psychol. Bull.*, 1952, **49**, 38–40.
3. Cohen, S. M. Thalamic loci of afferent stimulation in the cat. *Fed. Proc.*, 1950, **9**, 23.
4. Greenman, M. J., & Duhring, F. L. *Breeding and care of the albino rat for research purposes.* Philadelphia: Wistar Inst. Anatomy and Biology, 1931.
5. Liddell, H. S. Conditioning and emotions. *Scientific American*, 1954, **190**, 48–77.
6. Lindquist, E. F. *Statistical analysis in educational research.* Cambridge, Mass.: Riverside Press, 1940.
7. Weininger, O. Mortality of albino rats under stress as a function of early handling. *Canad. J. Psychol.*, 1953, **7**, 111–114.
8. Weininger, O. Physiological damage under emotional stress as a function of early experience. *Science*, 1954, **119**, 285–286.
9. Weininger, O. Morphological change under emotional stress as a function of early experience. Unpublished MS., Dept. of Psychology, Univer. of Toronto.

EXPERIMENTAL DEAFNESS

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THE observations to be reported here concern the "nonspecific" function of auditory stimulation in behaviour. In its specific function a sensory event arouses, guides, or inhibits a particular bit of behaviour: the S-R relationship. In addition, however, it appears that the sensory environment (with its constant variation of familiar elements in more or less familiar combinations) is essential to the maintenance of normal capacity for response, in the state referred to as "arousal" by the electrophysiologists.

Bexton, Heron, and Scott (1) found a demonstrable intellectual deterioration in college students beginning after approximately 24 hours of a rather drastic isolation from the perceptual environment. Their method, however, principally affected visual and tactual perception, and there was relatively little interference with auditory stimulation. The present experiment was designed to determine the generalized effects of a sharp loss in the auditory sphere alone, using Ramsdell's procedure as cited by Herb (2, pp. 252–3). This did not succeed, unfortunately, in completely eliminating air-borne sounds; but in the light of the findings reported by Bexton *et al.* the present results, though not extensive, appear to have some significance.

Ramsdell reported feelings of personal inadequacy and irritability, and under- or over-response in some social situations. The experiment was first repeated with one of the authors (E.S.H.) as subject, with results differing in certain respects from Ramsdell's but in general confirming his conclusions. Accordingly, the experiment was repeated with more subjects to get some idea of the extent of individual differences in the response to a sudden (partial) loss of hearing.

METHOD

Six college students (five male, one female) were paid to act as subjects over a three-day weekend. They were given no information about expected results, but were instructed to keep a diary recording

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anything out of the ordinary that they observed about themselves. The diaries, supplemented by interviews following removal of the earplugs, provide the data of the experiment. The subjects did not see one another during the experimental period.

The external auditory meatus was packed with cotton impregnated with petroleum jelly. This did not completely prevent the hearing even of speech. The subjects could with an effort converse with their friends; some had lectures to attend and were able to follow most of what the lecturer said (especially if they sat at the front of the room and paid careful attention to his lips), though they could not follow class-room discussions.

RESULTS

Specific Effects

All subjects experienced some physical irritation from the plugs, as itching or discomfort, but this seems to have been a major factor for only one subject, F. In the others, the degree of discomfort seemed not to be related to the degree of emotional change. Subjects D and E, for example, showed more emotional effects than A, B, and C, but reported that when they were studying on the day following the experiment they wished they had the earplugs back in place. The discomfort, evidently, was minor.

All subjects reported a persistent magnification of bone-conducted sounds (C: "I can hear some neck vertebrae creaking when I turn my head slowly. . . . I was surprised to find out how loud combing my hair is"). A and B both found themselves using exaggerated caution in doing things normally accompanied by noise, such as closing a door, or even laying a book on the table in the library where others were studying. Four subjects mentioned attempts at lip-reading, two reporting that this combined with residual hearing was better than residual hearing alone (although of course they had no training in lip-reading).

All six subjects reported an inability to speak with normal volume, which persisted throughout the three days of the experiment. As C put it, he could hear himself clearly when speaking, "but in a way that does not give me a clue as to how loud my voice is." Lack of such clues, however, cannot be the sole explanation; otherwise, underestimation of loudness would be as frequent as overestimation, and the subjects were unanimous that they persistently spoke in too low a voice. D reported, after attempting rehearsal of a play, "Though my voice fairly 'screamed' in my head, enough to distract me from playing the part, apparently my voice wasn't half loud enough." E was repeatedly annoyed by his friends' requests to speak louder. Another possibly significant observation: A's diary on the second day notes that "my coordination of speech is poorer than usual—I even missed a word once in a while in my sentence without noticing it." Nothing of this sort was reported by the others, although they were not asked specifically about it.

Nonspecific Effects

1. Motivation. In our preliminary experiment the subject (E.S.H.) had planned to give his four days to study, thus killing two birds with one stone. Ramsdell had not mentioned loss of motivation, and the subject was therefore surprised to find that he could not bring himself to study.

In our present experiment we failed to confirm this result clearly; in fact the opposite result was obtained with two of the six subjects. One subject, F, found study impossible, but we cannot rule out physical discomfort as the cause. Although the mode of insertion of the plugs was not expected to result in pain, F reported slight but persistent pain. Another subject, D, wrote of the University Library on the first day: "the complete dead hush here is terrible—enough to prevent work"; but had adapted on the two following days and studied as well as usual, or better. Two subjects did no studying, but said in the terminal interview that they had not planned to do so.

Since there was no doubt about the disturbance of motivation in our preliminary experiment, it is evident that there are marked individual differences in this respect, presumably related to different habits of study.

2. Emotion and attitude. Rather great individual differences were observed in other respects. Trivial emotional effects were reported by A and B, greater ones by C, and quite marked ones by D, E, and F. A had some feeling of inferiority ("I felt like a freak"), but no irritability or tendency to avoid others. B noticed occasional irritability only. The subjects of course explained the situation to their acquaintances, and all were able to converse by making an effort, so that there was no great barrier to maintaining social contacts.

C reported that he had strong feelings of personal inadequacy, but denied irritability or seclusiveness. His girl friend, however, did not agree with him at all; she described him as irritable and withdrawn during the whole experiment.

D, the only female subject in the experiment, seems to have had the male social situation well under control; at least she was not disturbed while in men's company. It was different with the women in her university residence. *First day*, dinner in the residence: "I don't feel like myself. I don't feel free to do and say things I ordinarily would. A feeling of constraint. I'm sure people think I'm terribly dense judging from their expression of doubt (this makes me feel very foolish and most anoyed)." *Second day*: "I have a panic that someone will say hello behind me or at the side and I won't be able to hear. . . . It makes me want to climb back into a shell. . . . I'll probably be accused of snobbery tomorrow." *Third day*, at lunch: "I made a table-wide comment to one and all–no one showed any signs of hearing it or acknowledging the remark. Did they hear it? Or was it not so wise? This could give one a complex. I feel this lack of hearing is giving me a snivelling personality."

E, *first day*: "Two friends walked into the room without my hearing them. I was startled. . . . My first reaction was one of anger." Friends persisted in asking him to speak louder, and this as persistently made him angry. *Second day*: He joined a crowd watching television in a store window. They began to laugh; "I couldn't see (or hear) what the joke was; I became annoyed and walked away." Also: "Becoming irritable–tonight a friend spoke to me on a subject which should not have aroused any anger–I answered sarcastically and stamped out of the room." *Third day*: "Becoming annoyed with people who keep asking me to speak louder. I resolved today not to leave my room in order that I wouldn't have to speak to people."

F was the subject who reported most physical discomfort, and also the one whose behaviour was most affected. As far as we can judge, however, the disturbance, with marked irritability and seclusiveness, was much greater than would be accounted for by the slight headache he described. In the terminal interview, for example, he reported that he had come near hitting a woman. This, he asserted, was not a habit of his. He was tired and sleepy throughout the experiment, but had trouble sleeping. It has already been noted that he was unable to study (though he had an examination on the day following the experiment). He did not ascribe the "lack of ability to concentrate" or his emotional changes to the physical discomfort; but this, together with the sleep disturbance, cannot be ruled out as the cause of the change of behaviour.

C, D, and E, however, appear to present clear evidence of a slight personality disturbance not accounted for by physical discomfort. Such disturbances might be expected to be more marked if the reception of air-borne sounds were suddenly and completely lost.

It is known that chronic deafness can accentuate pre-existing personality difficulties. We do not imply, of course, that deafness necessarily produces a generalized disturbance, even a mild one. Adaptation may be rapid (as in D's recovery of the ability to study on the second day), and when the condition comes on gradually there may not even be an initial disturbance. Our conclusion here is only that a sudden lowering of normal auditory input has shown clear evidence in this experiment, as in Ramsdell's, of a disturbance in behaviour that does not directly require auditory acuity for its guidance. The degree of this effect, and its form, may vary greatly from subject to subject.

REFERENCES

1. Bexton, W. H., Heron, W., & Scott, T. H. Effects of decreased variation in the sensory environment. *Canad. J. Psychol.*, 1954, 8, 70–76.
2. Hebb, D. O. *The Organization of behavior*. New York: Wiley, 1949.

AN EVALUATION OF RIGIDITY FACTORS[1]

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THIS study is the third in a direct line of investigation concerning the existence and nature of rigidity as a factor. The "rigid" person is defined as one who lacks the ability to perform *overlearned* operations in an unusual or unaccustomed manner; e.g., he might be expected to have difficulty in writing backwards or doing arithmetic backwards. When the word rigidity is used in the present paper, it will refer exclusively to this conception of rigidity. The reader generalizes at his own risk.

A distinction may be made between "motor" and "cognitive" tests of rigidity. Motor rigidity tests involve the performance of tasks demanding muscular or skeletal movements, whereas cognitive rigidity tests are of the problem-solving type. For example, a motor rigidity test might require the writing of *e*'s backward, while a cognitive rigidity test might comprise arithmetical problems in which multiplication signs meant division, addition signs subtraction, etc.

The questions with which the present paper deals may now be stated more precisely. Can a rigidity factor be identified? If so, will it be common to both motor and cognitive tests of rigidity? Or will separate factors appear, corresponding to the differences between motor and cognitive rigidity tests?

REVIEW OF PREVIOUS WORK

In the first study to be discussed here, Oliver and Ferguson (3) sought to demonstrate the existence of a cognitive rigidity factor. Their test battery included five tests designed by Oliver and Ferguson to measure cognitive rigidity, three tests of mental ability, and two other tests not of direct interest in the present discussion. It was hoped that some or all of the cognitive rigidity tests would define a cognitive rigidity factor. The mental ability tests were included to check the possibility that the assumed rigidity factor might involve reasoning components. Descriptions of the tests in this battery may be found in Oliver and Ferguson's article (3).

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[1] This paper reports part of a project carried out at McGill University under the auspices of the Defence Research Board, Ottawa. The writer wishes to express his gratitude for the guidance of Professor George A. Ferguson.

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These tests were administered to 98 McGill undergraduates and the results subjected to factorial analysis, using the methods of Thurstone (**5**). Centroid analysis yielded three factors which were then rotated into an oblique simple structure. Factor A was identified as a reasoning factor. Three of the five cognitive rigidity tests had high loadings on Factor B, which was identified as a cognitive rigidity factor. The other two cognitive rigidity tests had their highest loadings on Factor C, which was not interpreted.

In the second study to be discussed here, Scheier and Ferguson (**4**) attempted to determine the relation of motor rigidity tests to Oliver and Ferguson's cognitive rigidity factor. Scheier and Ferguson analysed a test battery composed of six cognitive rigidity tests, four motor rigidity tests, four motor speed tests, and two mental ability tests. The cognitive rigidity tests included the three measures of cognitive rigidity with the highest loadings on Oliver and Ferguson's cognitive rigidity factor, plus three newly devised cognitive rigidity tests. The motor rigidity tests were of the type traditionally used in studies of motor rigidity or "perseveration." They involved writing in reverse and printing mirror images of block or capital letters. The speed and mental ability tests were included as a check on the extent to which either motor or cognitive rigidity tests might involve these components.

These tests were administered to 60 undergraduates. Four centroid factors were extracted but examination of residuals suggested that only three of these factors should be included in the subsequent analysis. These three factors were rotated into an oblique simple structure. Reasoning, Cognitive Rigidity, and Motor Speed factors were identified. The cognitive rigidity factor was the one previously identified by Oliver and Ferguson. The so-called motor rigidity tests did not load on the cognitive rigidity factor, nor did they determine a motor rigidity factor of their own. Scheier and Ferguson (**4**, p. 29) write: " . . . the motor rigidity tests used in this study have a factorial composition which is very largely reasoning and motor speed." Motor speed accounts for most of the common factor variance in the motor rigidity tests. In fact, the motor *rigidity* tests have almost as high loadings on the motor speed factor as do the criterion motor *speed* tests. Apparently, the best way to predict how well a person will do a motor task backwards is to measure how well he does that task forwards. To this point we will return presently.

PROBLEM OF THE PRESENT STUDY

Thus far, results indicated the existence of a relatively narrow cognitive rigidity factor. It now seemed desirable to study the nature of this factor more intensively. Cognitive rigidity had seemed to be factorially distinguishable from mental ability as well as from motor rigidity and motor speed. But the mental ability realm had not been well represented in these studies. Oliver and Ferguson had only three mental ability tests in their battery; Scheier and Ferguson had only two. The present study attempted to remedy this situation by factorial analysis of a test battery which included cognitive rigidity tests and a wide selection of mental ability tests. The cognitive rigidity tests were those which the two previous analyses had indicated as the best available measures of cognitive rigidity.

THE TEST BATTERY[2]

Cognitive Rigidity Tests

Reversed Arithmetic Test. The subject was presented with simple arithmetical problems. But for the purpose of this test a plus sign meant subtract, a minus sign meant add, a multiplication sign meant divide, and a division sign meant multiply, the different operations being performed in the sequence in which they occurred in the problem. A sample item is: $3 \times 3 - 1 =$. The correct answer to this item is 2.

Reversed Alphabet Test. Oliver and Ferguson (3, p. 53) describe this test as follows: "The subject was asked to write the letter of the alphabet which came 2, 3, or 4 before the one listed, depending on the number written after the letter. For example, M – 3 signifies that the subject must write the letter in the alphabet that is three letters before M. This letter is J."

Reversed Reading. The subject was presented with a series of sentences of very simple meaning. The letters making up the words were in reverse order, right to left, whereas the words making up the lines followed each other normally from left to right. The subject was required to read each sentence and mark it true or false. Sample items are:

YREVE ERAUQS SAH RUOF SEDIS ....

EHT NUS NETFO SRAEPPA SA A NEERG LLAB ....

The correct answers to these items are "true" and "false" respectively.

Other Tests

The test battery also included 22 mental ability tests, among which were criterion variables for six of Thurstone's Primary Mental Abilities (PMA). Finally, there were three motor speed tests, involving speed of printing and writing.

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[2] It should be noted that this battery was designed to study problems other than those discussed in this paper.

ANALYSIS OF RESULTS

This test battery was administered to 100 undergraduates. Seven centroid factors were extracted and rotated into an oblique reference vector solution. The oblique vector solution is similar to the oblique factor solution in respect to *pattern* of loadings on any single factor or vector (1). Moreover, in the present study the oblique vector solution was also similar in loading pattern to a solution which seemed the best attainable approximation to an orthogonal simple structure. Since "pattern"-type information is most crucial in the present discussion, and since the oblique reference vector solution is much easier to achieve, this type of solution was used in the present study.

Six oblique vectors were interpreted, one in terms of motor speed and the other five in terms of various mental abilities. The cognitive rigidity tests were clearly not involved in the oblique speed vector. The vectors presented in Table I are the only ones on which all three cognitive rigidity tests had loadings.

TABLE I

VECTORS ON WHICH ALL THREE COGNITIVE RIGIDITY TESTS HAD LOADINGS

Oblique verbal reasoning vector		Oblique number ability vector	
Verbal Analogies	733	PMA Multiplication	934
Easy Verbal Analogies	732	PMA Addition	867
PMA Vocabulary	683	*Reversed Arithmetic*	576
PMA Completion	668	Easy Number Series	458
Reversed Reading	617	Letter Speed	382
PMA Letter Groups	602	Easy Figure Analogies	332
Word Classification	596	Number Series	326
Reversed Alphabet	485	*Reversed Reading*	302
Picture Classification	482	Number Speed	295
PMA Letter Series	448	*Reversed Alphabet*	258
Reversed Arithmetic	436		
Figure Analogies	408		
Easy Figure Analogies	399		
PMA Verbal Fluency	364		
Easy Letter Series	355		
Easy Number Series	330		
Number Series	323		

Obviously the cognitive rigidity tests are not the criterion variables for either vector. A major conclusion of this study must be, therefore, that cognitive rigidity tests are factorially complex, and do not determine a distinguishable cognitive rigidity factor of their own. Cognitive rigidity tests are apparently very largely tests of reasoning and other clearly recognizable mental abilities. One or more of the so-called cognitive rigidity tests were involved in every one of the five mental ability vectors isolated in this study. The reason that previous studies failed to reveal this factorial complexity is probably that these studies did not have adequate test coverage of the mental ability domain. It is true that there were also fewer cognitive rigidity criterion variables in the present study. But we must remember that these were the best available tests of cognitive rigidity, and that three good criterion variables should be enough to define a vector or factor.

If there are neither cognitive nor motor rigidity factors, what, if anything, *can* be said about rigidity? We have noted already that the motor rigidity tests are closely related to motor speed tests, which involve doing the same type of operations in a normal forward manner. Apparently much the same type of conclusion must be drawn for our cognitive rigidity tests. Consider first the vector loadings in Table I. The arithmetic rigidity test is the third-highest-loading variable on the number ability vector, just behind Thurstone's arithmetic tests which are the criterion variables for this factor. In other words, the man who does arithmetic best backwards tends to be the man who does it best forwards. The same sort of thing is indicated by the fact that reversed reading and reversed alphabet are higher on the verbal vector than is reversed arithmetic.

Now the Reversed Alphabet Test seems to require the ability to use the alphabet backwards. The Letter Groups, Letter Series, and Easy Letter Series tests seem to require the ability to use the alphabet in a normal way. The rough data of Table II indicate how closely these four alphabet tests are related factorially.

Ability to use the alphabet backwards seems closely related to ability to use the alphabet forwards. In general, then, cognitive rigidity tests, although seeming to require the performance of a given class of mental ability operations in a reversed or unusual manner, may actually measure mental ability components closely related to normal forward performance of these operations.

CONCLUSIONS AND INTERPRETATIONS

The collated results of three factorial investigations of rigidity seem to justify the following conclusions:

1. The existence of a cognitive rigidity factor is not confirmed. Cognitive rigidity tests appeared to determine their own factor in the two earlier studies only because there was not sufficient concurrent coverage of the mental ability realm in these studies.

2. Cognitive rigidity tests are largely measures of other clearly defined mental abilities.

TABLE II

THE RANKS OF ALPHABET TEST LOADINGS ON THE VECTORS

Test	Rank of loading
Verbal reasoning vector	
PMA Letter Groups	6
Reversed Alphabet	8
PMA Letter Series	10
Easy Letter Series	14
Series reasoning vector	
Easy Letter Series	2
Letter Series	3
Reversed Alphabet	6
PMA Letter Groups	7
Memory vector	
Easy Letter Series	6
Reversed Alphabet	7
Spatial vector	
Easy Letter Series	4
Reversed Alphabet	9
Letter Groups	10

3. The existence of a distinct motor rigidity factor is not confirmed.
4. Motor rigidity tests are largely measures of motor speed and, to a lesser extent, of mental ability.
5. Where *overlearned* motor or cognitive operations are concerned, the ability to perform an operation backwards (or in an unusual way) seems most closely related to the ability to perform the same class of operation in a normal manner.

These conclusions, especially the last one, may merely indicate that we have not yet been able to devise adequate tests of rigidity. But, in the writer's opinion, any radical change in these tests would not make them better tests of "rigidity" but rather measures of something not presently included in our theoretical conception of rigidity. Rigidity otherwise defined and measured might yield a rigidity factor.

The importance of being able to predict rigid behaviour is unaltered by the fact that rigidity is not a distinct factor. Many industrial or military jobs may require the performance of overlearned operations in some new or unusual manner. Where this is the case, adequate personnel selection requires that we be able to predict rigidity of behaviour in given types of operations. The present study has an important bearing on this problem. It suggests that in many cases predicting rigidity may not require the construction of special new tests of rigidity. One has only to measure the subjects' performance on already extant motor or mental ability tests involving the class of operation in question. Normal or forward performance of a class of operations will be the best predictor of unusual or reversed performance of this type of operation. In short, the present study suggests that the *type* of mental or motor operation (writing *d*'s, arithmetic, etc.) is more important than the *direction* of mental or motor operation (forward or backward) in determining performance.[3]

REFERENCES

1. Cattell, R. B. *Factor analysis*. New York: Harper, 1952.
2. Luchins, A. S. Mechanization in problem solving. *Psychol. Monogr.*, 1942, **95** (Whole No. 248).
3. Oliver, J. A., & Ferguson, G. A. A factorial study of tests of rigidity. *Canad. J. Psychol.*, 1951, **5**, 49–59.
4. Scheier, I. H., & Ferguson, G. A. Further factorial studies of tests of rigidity. *Canad. J. Psychol.*, 1952, **6**, 18–30.
5. Thurstone, L. L. *Multiple factor analysis*. Chicago: Univer. of Chicago Press, 1947.

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[3] It should be noted that the close functional relation of backward and forward operations of a given class is meant to apply to overlearned operations, not to backward or forward performance relevant to experimentally induced behaviour patterns. That is, the statement is not meant to apply to rigidity conceived of as "the interfering effects of an experimentally-induced behavior pattern" (**3**), a conception of rigidity exemplified by Luchins' work on the Einstellung effect (**2**).

INTERPERSONAL PERCEPTION AND MARITAL HAPPINESS

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In the past few years there has been a burgeoning of interest in an area which can be loosely designated as that of interpersonal perception. Two of the variables which have been studied are "empathy," or the extent to which one individual perceives another as the latter perceives himself (**3**, **4**, **6**), and "assumed similarity" (**7**, **8**, **9**), the congruence between the descriptions given by one person (*a*) of himself, and (*b*) of others whom he is asked to describe. There has been some attempt to relate these variables to measures of group functioning, but neither the ability to predict others' self-descriptions nor the ability to predict a group's opinions seems to be related in any consistent way to the position one occupies in a group (**2**, **3**, **6**, **10**, **11**, **13**). And evidence on the relation between the degree of assumed similarity among group members and their effectiveness as a team is very mixed (**5**).

Although it seems logical to suppose that a high degree of reciprocal understanding might make for more effective group functioning, this hypothesis appears still be to just that—an hypothesis. One obvious dimension in this problem is the kind of group involved, i.e., the degree to which effective functioning depends on mutual understanding. The interpersonal unit which seems to stand at one extreme in this respect is the married couple, and marriage, accordingly, should be a crucial area in which to test for the understanding-effectiveness relation. The problem of a criterion for the functioning of marital units was dealt with, in the present study, by equating effective functioning with happiness of the marriage as rated independently by each partner. The rationale of this equation is that happiness is the marriage value held most strongly in this culture.

Much of the literature on marriage stresses the importance to happiness of similarity of the partners (**1**). Presumably it is easier to understand and therefore accept a person who is like you, or whom you assume to be like you, than one who differs. The present study examines both the similarity of the couple's self concepts, and also the degree of understanding of the spouse's self concept, to discover whether it is similarity (real or assumed) *per se*, or accurate understanding of the

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partner (whether he be similar or not), which is most closely related to happiness of the marriage.

To put the problem more succinctly, it is to investigate, in a small group of marriage partners:

1. The relation of the *understanding* which each has of the other's self conception (as measured by accuracy of prediction) to happiness of the marriage (as rated by the marriage partners and checked by an objective judge).
2. The relation of the *similarity* of the couple's self conceptions to the subjective happiness rating.
3. The relation of *assumed similarity*, or projection of one's own characteristics onto the spouse, to the happiness rating.

PROCEDURE

Fifteen couples were chosen who were all well known to the author. The length of time married ranged from six months to thirty-seven years, and the mean was 10.4 years.

To provide the happiness criterion, each member of the couple was asked to list independently the names of 10 married couples well known to him and his spouse. He was then asked to rank these from 1 to 10, according to his judgment of the happiness of the marriage. Next he was asked to name the couple whose marriage his own most closely resembled. His ranking of this couple was then taken as his ranking of his own marriage. The couples in the sample were also ranked for happiness by the investigator, who knew each couple personally.

Each partner was then required to fill out a true-false questionnaire made up of 115 Minnesota Multiphasic Personality Inventory items. One hundred of these statements were selected for their relevance to interaction with others; the remaining 15 were the Lie Scale items. After each had completed his own questionnaire he was asked to fill out another copy as he predicted his spouse would answer it.

Thus for each couple there were four true-false endorsements of each item. For example:

Items:	*1*	2	3	*4*	5 (etc.)
1. Husband's own answer	T	T	F	T	F
2. Husband's prediction of wife's answer	T	F	F	T	T
3. Wife's own answer	F	T	F	T	T
4. Wife's prediction of husband's answer	T	T	T	F	F

From these four sets of answers, scores can be derived for understanding (accuracy of prediction), similarity, and assumed similarity—the three variables chosen for investigation.

In the example above, the husband's *understanding* score can be computed by comparing rows 2 and 3. Here the husband accurately predicted the wife's answers on items 3, 4, and 5, and would score three out of five. The wife's understanding score is similarly derived by comparing rows 4 and 1; she also scored three out of five. The *similarity* score for this couple is the total number of identical answers given in rows 1 and 3; in this case they gave the same answers to items 2, 3, and 4. The *assumed similarity* score is derived for each partner separately by comparing his own answers with his predictions of his spouse; i.e. for the husband: rows 1 and 2, items 1, 3, and 4; for the wife: rows 3 and 4, item 2.

On the basis of the happiness criterion the group was then divided into eight "happy" couples (who picked, as most resembling their own marriage, one they had ranked between 1 and 3) and seven "unhappy" couples (ranked 4 to 10). In only two cases did the investigator's ranking play a part; in one case where the husband ranked the marriage "unhappy" and the wife's rating was "happy," and in another where both partners refused to rank their marriage. In each case the investigator's rank was "unhappy" and the couple were put into the unhappy group. In all other instances the three rankings fell within the same range, "happy" or "unhappy" as the case might be.

One of the technical problems in studies of individual predictive accuracy is that the understanding or "empathy" score may reflect not only the ability to predict a particular other but also ability to predict how the item would be marked by most people in one's group, i.e. knowledge of the stereotyped reply. This difficulty has been pointed out by Lindgren and Robinson (**12**), Dymond (**5**), and others. The problem was met in this study by eliminating all items which were marked either "true" or "false" by 66 per cent or more of the group. This left 55 items on which the subjects' responses differed to a reasonable extent; accuracy of prediction on these may presumably be attributed to factors other than knowledge of the stereotyped reply.

RESULTS

The first hypothesis, that happier couples would have more understanding of each other as measured by their accuracy scores, was confirmed. Since the accuracy scores conformed to a normal curve reasonably well, and since t is the most sensitive measure to apply to small numbers, the difference between the means of the total accuracy scores of the happy and unhappy groups was tested by t and found to be significant at the 1 per cent level (mean accuracy scores: happy group, 38.17; unhappy group, 32.93). The rho correlation between happiness and accuracy is .44 which is also significant (for 29 degrees of freedom). In both groups the proportion of accurate to inaccurate predictions exceeded chance expectancy.

The evidence on the second question is also positive. The happy couples were, as a group, significantly more alike in their self descriptions than the unhappy couples. The difference between the means of the number of items answered similarly by both members of the marriage unit was significant at the 1 per cent level using *t* (mean similarity scores: happy group, 32.37; unhappy group, 25.14). The rho correlation between real similarity and happiness is, however, not significant for 14 degrees of freedom ($\rho = +.419$).

The tendency for happy couples to check items similarly raises the question: are their higher scores for accuracy of prediction (accuracy scores) then misleading? Since the happy spouse actually answers many questions in the same way as his mate does, he has a higher chance than the unhappy spouse of being accurate through "projection," or assumed similarity, rather than understanding. However, correlation of the number of items checked identically by couples with their accuracy scores showed less association of similarity and accuracy in the happy group ($\rho = .13$) than in the unhappy group ($\rho = .72$). Whereas the correlation of similarity and accuracy is not significant for the happy group, it is for the unhappy group. Thus the alternate explanation of the happy group's higher accuracy scores is directly refuted.

To turn now to the third question, the relation of assumed (i.e. predicted) similarity to happiness and to accuracy, all evidence seems negative. There is no significant difference between the happy and the unhappy group means for the number of predictions made of their spouses which correspond to their own replies (assumed similarity means: happy group, 33.70; unhappy group, 33.07).

The correlations of assumed similarity scores and accuracy scores are insignificant for both groups; however, when the *kinds* of error made by the two groups are compared, a real difference appears. Two types of error are possible: (1) where an actual similarity between self and spouse exists but a difference is predicted; and (2) where the spouse is assumed to be similar when he actually differs. The happy group made errors of both types with about equal frequency, as seen in Table I (type 1, 7.75; type 2, 9.08). The unhappy group, on the other hand, made more errors of the second type (type 1, 7.07; type 2, 15.00), the difference being significant at the 1 per cent level, using *t* for correlated means. The difference between the two groups in number of type 2

TABLE I

RELATION BETWEEN REAL AND PREDICTED (ASSUMED) SIMILARITY AND DIFFERENCE IN OWN REPLIES OF HAPPY AND UNHAPPY MARRIED GROUPS

	Happy			Unhappy		
	ASSUMED			ASSUMED		
	Similarity	Difference	Total	Similarity	Difference	Total
Real	(correct)	(type 1 error)		(correct)	(type 1 error)	
Similarity	24.62	7.75	32.37	18.07	7.07	25.14
	(type 2 error)	(correct)		(type 2 error)	(correct)	
Difference	9.08	13.55	22.63	15.00	14.86	29.86
Total	33.70	21.30	55.00	33.07	21.93	55.00

errors (unwarranted assumption of similarity) is also significant at the 1 per cent level, using *t*.

Another interesting difference appears from inspection of Table I. The happy group estimate the excess of their mutual similarities over their differences at 33.70 — 21.30, or 12.40. This is close to the figures for the real similarities and differences (32.37 — 22.63, or 9.74), the error being only 2.66 and the difference not significant. The unhappy group, however, estimate the excess of similarities over differences at 11.44, whereas actually the differences exceed the similarities by 4.72. This difference is significant at the 1 per cent level. The error of estimate made by the unhappy group is 16.16; the difference between it and that made by the happy group (2.66) is significant at the 1 per cent level.

The relation between length of time married and the level of predictive accuracy was also investigated. The correlation was about zero ($\rho = .004$). Time alone does not seem to increase a person's ability to predict his mate's self perceptions. This is logical in that happiness and length of time married also correlated insignificantly ($\rho = +.21$).

Another interesting sidelight is that husbands and wives are able to predict each other's replies about equally well ($\rho = .79$).

Reliability and Validity

The split-half reliability, calculated by correlating accuracy on first half of the test with accuracy on the second half, was .927, high enough for acceptance of the reliability as adequate. The validity seemed demonstrated by two facts: (1) the concurrence of the investigator's happiness ranking with the subjects' own rankings, and (2) two items directly pertinent to marital happiness were answered significantly differently by the happy and the unhappy groups. These two items were:

"I believe my home life is more pleasant than that of most people I know."

"I have very few quarrels with members of my family."

To both these questions the happy group answered "true" significantly more often than the unhappy, the differences being significant at the one per cent level.

Item Differences: Happy vs. Unhappy

Very few of the 115 items taken individually were answered significantly differently by the two groups, using χ^2 with Yates's correction. The 15 items which showed the greatest differences are shown in Table II. Taken together, these gave a difference significant at the 5 per cent level. Although the groups included in this study are much too small to warrant generalizing, these 15 items suggest personality differences which might deserve investigation with a larger sample. Unhappy couples appear less at ease socially, more tense, and less able to communicate spontaneously with others. Happy couples seem to have more social interest and facility, and less hesitation in speaking up about things that are on their minds. The items most characteristic of the unhappy describe them as worried, sensitive, shy; those characteristic of the happy describe them as forthright, able to confide in others, social, patient, capable of meeting their problems.

These tentative characterizations would readily explain why unhappy partners have more difficulty in predicting the replies of their spouses; they have less interest in other people, and are therefore less attentive to cues given by others; in addition, the cues are probably fewer if the spouse too "feels unable to tell anyone all about himself" and "does not like to let people know where he stands on things."

SUMMARY

Thirty subjects, comprising 15 married couples, were tested for their ability to predict their respective spouses' responses to 55 items from the MMPI scale. Their scores were then related to the happiness of the marriage, as rated by the marriage partners and by an outside judge. The happily married group were significantly more accurate in their predictions than were the unhappily married. The same questionnaire was also filled out by each subject for himself, and the answers of happily married spouses resembled each other more than did those of the unhappily married.

In predicting their partners' responses the unhappy group significantly exceeded the happy in the proportion of errors predicting similarity of

TABLE II

ITEMS WHICH DIFFERENTIATE HAPPY AND UNHAPPY GROUPS

	Happy		Unhappy		Signif.
	True	False	True	False	%
1. I believe my home life is more pleasant than that of most people I know.	16	0	8	6	1
2. I have very few quarrels with members of my own family.	16	0	7	7	1
3. I often prefer to pass by school friends or people I know but haven't seen for a long time unless they speak to me first.	2	14	7	7	5
4. I have had periods when I lost sleep over worry.	7	9	11	3	3
5. I am more sensitive than most other people.	6	10	10	4	15
6. I frequently have to fight against showing I am shy.	5	11	9	5	15
7. People often disappoint me.	3	13	7	7	15
8. I am a good mixer.	11	5	6	8	15
9. When someone says silly or ignorant things about something I know about I try to set him right.	13	3	7	7	15
10. I easily become impatient with people	4	12	8	6	15
11. I feel unable to tell anyone all about myself.	5	11	8	6	N.S.
12. While on trains, busses, etc., I often talk to strangers.	10	6	6	8	N.S.
13. I like to let people know where I stand on things.	10	6	6	8	N.S.
14. I am easily embarrassed.	5	11	8	6	N.S.
15. I shrink from facing a crisis or difficulty.	6	10	9	5	N.S.

response where a real difference existed; that is, members of the unhappy group tended to underestimate the differences between themselves and their partners. Questionnaire items which discriminated between the two groups served both as validation of the happiness criterion and as indicators of personality factors associated with good or poor marital relations.

The findings on this small group of married couples appear to confirm the general hypothesis that happiness of a marriage is related to the partners' understanding of one another, as reflected in their ability to predict each others' responses to a series of items on a personality inventory. In other words, married love is not blind, and ignorance is not connubial bliss. The better each partner understands the other's perceptions of himself and his world, the more satisfactory the relationship.

REFERENCES

1. Burgess, E. W., & Cottrell, L. *Predicting success and failure in marriage.* New York: Prentice-Hall, 1939.
2. Chowdry, K., & Newcomb, T. M. The relative abilities of leaders and non-leaders to estimate opinions of their own groups. *J. abnorm. soc. Psychol.*, 1952, **47**, 51–57.
3. Dymond, Rosalind. A scale for the measurement of empathic ability. *J. consult. Psychol.*, 1949, **13**, 127–133.
4. Dymond, Rosalind. Personality and empathy. *J. consult. Psychol.*, 1950, **14**, 343–350.
5. Dymond, Rosalind. Can clinicians predict individual behaviour? *J. Pers.*, 1953, **22**, 151–161.
6. Dymond, Rosalind, Hughes, Anne, & Raabe, Virginia. Measurable changes in empathy with age. *J. consult. Psychol.*, 1952, **16**, 202–206.
7. Fiedler, F. A method of objective quantification of certain counter-transference attitudes. *J. clin. Psychol.*, 1951, **7**, 101–107.
8. Fielder, F., Blaisdell, F., & Warrington, W. *Unconscious attitudes and the dynamics of sociometric choice in a social group.* Technical Report No. 1, Contract N 6 ori-07135, between the University of Illinois and the Office of Naval Research.
9. Fiedler, F., Hartman, W., & Rudin, S. A. The relationship of interpersonal perception to effectiveness in basketball teams. Technical Report No. 3, Contract N 6 ori-07135, between the University of Illinois and the Office of Naval Research.
10. Gage, N. L., & Suci, G. Social perception and teacher-pupil relationships. *J. educ. Psychol.*, 1951, **42**, 144–153.
11. Hites, R., & Campbell, D. A test of ability of fraternity leaders to estimate group opinions. *J. soc. Psychol.*, 1950, **32**, 95–100.
12. Lindgren, H., & Robinson, Jacqueline. An evaluation of Dymond's test of insight and empathy. *J. consult. Psychol.*, 1953, **17**, 172–176.
13. *Studies in naval leadership.* Technical Report Project 268. Personnel Research Board, Ohio State University. Columbus: Ohio State University Research Foundation, 1949.

BOOK REVIEWS

A Primer of Sociometry. By MARY L. NORTHWAY. Toronto: University of Toronto Press, 1952. Pp. vi, 48. $2.25, cloth bound; $1.50, paper bound.

THIS book is modestly called a primer because "it introduces the student to the basic principles and practices of sociometry and guides him gently into the intricacies of the literature in the field," but it does much more than this, and merits the attention of the experienced worker as well as the novice. Its significance lies not only in its value as a practical handbook and theoretical treatise, but in its contribution as a summary of what has been accomplished, and as a preview of what must yet be done. It is research-oriented. The reader is given new theoretical insights, and is constantly reminded of unsolved problems and of hypotheses to be investigated.

The instructions offered for the construction, administration, and scoring of sociometric tests, and the advice given for graphically portraying and interpreting results, are based on the author's own extensive experience and that of some of the most authoritative workers in the field. Although statistical techniques are not emphasized, reference is made to the various methods which have been devised for handling these types of data. Specific references are provided throughout. This material should help the beginning student to avoid the errors in procedure which have invalidated the findings of a number of previous investigators.

In the discussion of the interpretation of scores the reader is warned against regarding a sociometric score as a measure of popularity or as a measure of mental health. A high score does not indicate that a person is universally liked nor does a low score mean that he is universally disliked. Those who have low scores are just unchosen or rarely chosen. A larger number of seriously disturbed persons have been found in the high and low score groups than in the medium scoring group. It is suggested that the ability to form reciprocal relationships may be more closely related to personal security. It would appear that a high sociometric score simply reflects a greater sensitivity to the social group and a tendency to direct energy toward the activities of which the group approves.

Both the limitations and the untested possibilities in sociometry are considered. Statistically equivalent scores do not represent identical patterns of choice. It would appear that an examination of sociometric patterns would be more significant and fruitful than an analysis in terms of scores. No satisfactory method has been developed for measuring the intensity of relationships, but some success has been achieved in the measurement of the influence or power of an individual in his group. While most investigators have focused their attention on the chosen, Dr. Northway suggests it might be more appropriate to study the chooser. In reality, sociometry measures psychological choice, and since "social value, like beauty, is in the eyes of the beholder" the reasons for choosing may be more significant than the reality which is chosen. It is suggested that sociometry may provide a better measure of social want than of social value.

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Who Shall Survive? Foundations of Sociometry, Group Psychotherapy and Sociodrama. By J. B. MORENO. Beacon, N.Y.: Beacon House Inc., 1953. Pp. cxiv, 763. $10.00.

IT is unusual for a man to write two books with the same title. Moreno has done so. *Who Shall Survive? A New Approach to the Problem of Human Interrelations*, published by the Nervous and Mental Disease Publishing Company in 1934, was a monograph describing the use of sociometric techniques in a girls' training school. It marked the birth of sociometry in America. The 1953 book, which is not indicated as a second edition, is a systematic summary of the accomplishments of sociometry, group psychotherapy, and sociodrama as these are viewed by the mind of Moreno. He discusses sociometry not merely as a technique but as a theoretical basis for the clinical developments of sociodrama and group therapy. He also considers it the essential foundation for understanding the structure of society. The book includes findings from most of the important studies of the past twenty years. Although direct reference to the specific investigators is rarely given, an intensive bibliography shows the great number of scientists who have been involved. This book will be an essential encyclopaedia for all future workers in this area.

Moreno includes a section called "Preludes" (114 pages) in his new book. This is partly an autobiography and partly the biography of the sociometric movement. This may clarify the current confusion between sociometry as science and sociometry as doctrine, at least by revealing its sources. Moreno's statements, such as "An idea book like *Who Shall Survive* cannot be conceived in collaboration," and "I have written two bibles, an old testament and a new testament," indicate why sociometry has had difficulty being accepted among the scientific brethren; they also reveal why, in spite of the fact that within its own ranks there have been so many dissenters, the orthodox sociometry army has succeeded in militantly marching on, saluting the banners of the Leader.

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Method and Theory in Experimental Psychology. By CHARLES E. OSGOOD. Toronto: Oxford University Press, 1953. pp. vi, 800. $10.50.

AMONG those who will welcome this book are instructors who have tried to shape a course in experimental psychology suitable for helping advanced undergraduates to translate systematic issues into research. It will greatly reduce the problem of choice of material and also provide most of the essential interpretation. This is, however, more than a textbook. It is a reference work in which the author has avoided encyclopaedic citation of experiments and the tedium of a handbook. Osgood has made a broad but representative selection, within his framework of interpretation, from the experimental literature on sensation, perception, learning, and the symbolic processes, and has achieved a nice integration of description of method, assessment of outcomes, and appraisal of theories. As might be expected, the field of learning has received the most attention. But it would be misleading in such a brief review to attempt to deal with specific content. It is unlikely that the continuity and balance of the material in each of the four parts could have been attained by the convenient device of multiple authorship. The book is well documented (1,300 references) and judiciously illustrated. In places the reader would perhaps welcome a more detailed account of technique, but he cannot expect to find, within the available space, a manual on apparatus and experimental design. The term "method," therefore, means "general procedure," and the descriptions of procedure are assimilated to the exposition of experimental findings. It may also appear that the interpretation of research results has been unduly assimilated to the author's point of view. This will be a matter of opinion, that of the reviewer being that this assimilation is a distinct advantage, to the extent that it is true. By the same token, it is felt that the operational emphasis repeatedly convinces the reader that this is something he himself can do. This excellent book gives no support to the viewpoint that one can rarely be effective in both experimentation and theory.

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Rorschach's Test III—Advances in Interpretation. By SAMUEL J. BECK. Toronto: Ryerson Press 1953, Pp. viii, 301. $6.50.

THIS third volume by one of the original exponents of the Rorschach method reports and illustrates his advances in its interpretative use since 1945. Beck states in his preface that the advances are based, in part, on two research projects in which he has been engaged for the past five years; and, in part, they result from continued exposure to the clinical method. In this book the author deals with both quantitative and qualitative aspects of the test, and follows the pattern of his previous two volumes by liberally illustrating his points with actual records and clinical material.

In the first chapter, Beck reviews various approaches which have contributed to the study of personality and the development of the Rorschach method. He points out the general shifting of interest to ego psychology, and his feeling that interpretation of the Rorschach must emphasize the ego strength and defence mechanisms.

In the second chapter, the author sets out his findings regarding the significant interpretative factors in evaluating the ego, the defence mechanisms, and the effectiveness of their functioning in terms of the whole individual. In doing this, Beck introduces his adaptation of the Levy Movement Scale for giving relative weighting to various types of movement responses.

In his final chapter Beck indicates some of the limitations of the Rorschach technique and concludes with a plea for more thorough investigation of the total personality. However, he criticizes current research in respect to constancy of test technique and the underlying statistical methods whereby the test variables are being tried out.

Chapters III to VI are devoted to the case histories and Rorschach protocols. These are given in detail and carefully analysed, and provide good illustrations of the author's approach. If one might point to a weakness, it is his concluding generalization concerning anxiety and schizophrenia, which does not seem justifiable by the data presented.

This third volume admirably fills the need to have set forth in a lucid and definite manner the advances in the interpretation of the Rorschach which emphasize the ego. This is in keeping with an increased understanding of the dynamic functioning of the personality: the student of personality testing and projective techniques should find the book especially valuable.

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BOOKS RECEIVED*

Arbous, A. G. *Tables for Aptitude Testers.* Johannesburg: National Institute for Personnel Research, 1952. Pp. iii, 86.

Baumgardt, David. *Philosophical Periodicals.* Washington: Library of Congress, 1952. Pp. vi, 89.

Broad, C. D. *Religion, Philosophy and Psychical Research.* Toronto: British Book Service, 1953. Pp. vii, 308.

Dollard, John, Auld, Frank Jr., & White, Alice Marsden. *Steps in Psychotherapy. Study of a Case of Sex-Fear Conflict.* Toronto: Macmillan, 1953. Pp. ix, 222.

Fairbairn, W. Ronald D. *Psychoanalytic Studies of the Personality.* Toronto: British Book Service, 1952. Pp. xi, 312.

Halpern, Florence. *A Clinical Approach to Children's Rorschachs.* Toronto: Ryerson, 1953. Pp. xiii, 270.

Ingle, Dwight, & Baker, Burton, L. *Physiological and Therapeutic Effects of Corticotropin (ACTH) and Cortisone.* Toronto: Ryerson, 1953, Pp. xii, 172.

James, H. E. O., & Tenen, Cora. *The Teacher Was Black.* Toronto: British Book Service, 1953. Pp. v, 120.

Jones, Ernest. *Sigmund Freud. Life and Work: Vol. 1 The Young Freud (1856–1900).* Toronto: Clarke, Irwin, 1953. Pp. 454.

Jones, Maxwell. *Social Psychiatry. A Study of Therapeutic Communities.* Toronto: British Book Service, 1952. Pp. xix, 186.

Naumberg, Margaret. *Psychoneurotic Art: Its Function in Psychotherapy.* Toronto: Ryerson, 1953. Pp. x, 148.

Nuttin, Joseph. *Psychoanalysis & Personality.* New York: Sheed & Ward, 1953. Pp. xiv, 310.

Piers, Gerhart, & Singer, Milton B. *Shame and Guilt. A Psychoanalytic and a Cultural Study.* Toronto: Ryerson, 1953. Pp. x, 86.

Redfield, Charles E. *Communication in Management.* Chicago: University of Chicago Press, 1953. Pp. xvi, 290.

Smith, May. *An Introduction to Industrial Psychology.* Toronto: British Book Service, 1952. Pp. 292.

Snapper, I., Turner, Louis B., & Moscovitz, Howard L. *Multiple Myeloma.* Toronto: Ryerson, 1953. Pp. vi, 168.

Sprott, W. J. H. *Social Psychology.* Toronto: British Book Service, 1952. Pp. xiv, 268.

Wortis, Joseph (Ed.). *Basic Problems in Psychiatry.* Toronto: Ryerson, 1953. Pp. v, 186.

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*The inclusion of a book in this list does not necessarily preclude a review of it appearing in a later issue of the *Canadian Journal of Psychology.*